# THE APRIL SCIENTIFIC MONTHLY

EDITED BY J. McKEEN CATTELL



THE SCIENCE PRESS

LANCASTER, PA.—GRAND CENTRAL TERMINAL, N. Y. CITY—GARRISON, N. Y.

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# THE SCIENTIFIC MONTHLY

APRIL, 1930


## THE SOCIAL SCIENCES TO-DAY

PRESIDENTIAL ADDRESSES BEFORE THE AMERICAN ECONOMIC ASSOCIATION, THE AMERICAN STATISTICAL ASSOCIATION AND THE AMERICAN SOCIOLOGICAL SOCIETY AT A JOINT MEETING IN WASHINGTON, D. C. DECEMBER 28, 1929

# HISTORICAL RECORDS


By Dr. EDWIN F. GAY

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**Economic history belongs to both history and economics; it seeks to be helpful to both but to dominate neither. By its own relationship to these two disciplines it illustrates the growing cooperation of the social sciences, of which a specific recent example is the proposed international investigation of price history. But economic history suffers from the defect of all history, the imperfect character of the historical record and the various biases in its interpretation. The record tends to preserve evidence of institutional forms, while processes of change in the ideas which underlie institutions are much more evanescent. The increasing minuteness of modern social recording, especially its quantitative measurements, and the new zeal in collecting business documents will help to complete the record for the future economic historian. The demand for a great amplification of statistical records is a part of the contemporary triangulation, now proceeding, upon which to base a new development of social controls.**


If the shoemaker is well advised to stick to his last, the learned professions, which in our day have inherited much of the medieval craftsman's ethics, may occasionally obey this precept. I may properly, therefore, on this occasion make some random observations upon historical records, the raw materials of my craft. Practically all the great variety of records used by historians in general are also necessary to the economic historian, and his technique in the use of these records is that common to all historical criticism. But this branch of history has chosen for its field that longitudinal section through the great mass of material which is concerned primarily with economic activities and relationships. Since the economic historian is vitally interested in the past development of economic institutions and processes and their bearing on present problems, he must also regard his work as a branch of the science of economics. Fundamentally important, however, as the economic factor appears to him, he does not regard it as the one determining factor in human affairs past or present. Man does not live by bread alone. And no active group of economic historians is to-day making any such claim for the predominance of this specialism as did the German "historical school," from Roscher and Hildebrand to Schmoller.

The leaders of the "historical school" inevitably had to err by excess of zeal in

order to make their contribution in establishing the principle of relativity; no longer can any one proclaim, as did the Manchesterists of the mid-nineteenth century, that free trade is a doctrine eternally valid and universal in its application. The economic historian of our time realizes that the problems, economic and social, which confront us are much too complex and difficult to be understood by any one-sided approach. The cooperation of every usable method is required; and the critical method of history, emphasizing sequential relationships, is one among several essential tools. Like the physical sciences in their modern interlocking, the social sciences are borrowing from one another both tools and workers.

The historical investigation of prices now being launched, with the aid of a substantial grant from the Rockefeller Foundation, involves such collaboration. This history of prices, including wages, from the earliest fairly continuous records to that point, differing of course in each country studied, where the various price series may be linked with those in current statistical use, is to be carried on by scholars from England, France, Germany, Austria, Spain and the United States, under the chairmanship of Sir William Beveridge. Within the next five years, the establishment of long measures of price-changes in these countries may be expected. It is hoped to document these more fully and make them more reliable than was possible with those produced by such pioneers, working single-handed, as Thorold Rogers, d'Avenel or Wiebe. Price records will be sought which are continuous for one locality or for homogeneous market areas. Basic price series thus set up may of course be combined to show larger regional or national averages, but the totals will not conceal the divergent weightings of different regional price levels. A uniform general plan will be followed, including a careful reduction of the numerous local variations in weights, measures and coinage, so that some approximation may be made to international comparisons—a procedure of especial importance, for instance, in studying the geographical dispersion of price-changes as the great wave of the sixteenth century price revolution spread from Spain over western Europe. By separating and comparing the various groups of prices, new light may be cast on long-time changes in the behavior of prices and on the effect of such changes upon the attitudes and positions of great social groups of the population at successive periods. The investigation will require not only the association of scholars from the several nations selected, but also the close cooperation of a number of workers in different fields, especially of historians, archivists, economists and statisticians. When once this relatively dependable causeway has been thrown over the morasses of earlier economic history, we shall have greatly lengthened, though on a narrow front, our statistical base-line, now so short.

But happy as are the economic historians over this opportunity for the improvement of their one possible long statistical measure, they realize fully how imperfect their best efforts must leave it, for the causeway will be very narrow, and at places badly broken. Even with the cautious piecing together of much more material than has hitherto been available, serious gaps and deficiencies in the historical record must inevitably remain.

Although always aware of the serious deficiencies in all the records upon which he is dependent, the historian, by reason of the requirements of continuous narrative, usually fails to convey to the reader the full meaning of this imperfection. Absence or scarcity of records, often at vital points and over considerable spaces of time, is the most obvious and distressing of handicaps. But continuously baffling is the qualitative in-

adequacy of the material, its inherent and shifting biases, even when, as in the most recent centuries of history, the volume of records increases enormously. The rise of modern states and of centralized governments resulted in an immense increase of paper-work and the gradual organization of state archives, where there are housed and prepared for use great collections of political, administrative and judicial documents. The invention of the printing-press brought a flood of books and pamphlets, and by its help in making men literate brought forth new stacks of writings, private in character, to bless and to harass the historian. To curse with Carlyle this Dryasdust accumulation or to wish that the multitude of men had remained voiceless is inadmissible. Instead, the crushing task is divided and subdivided by shorter periods and narrower topics, but the increasing division of labor has not lessened the most crucial difficulties.

This very defectiveness of the raw material of his craft has largely determined and limited the method and aim of the historian. Historical technique, higher criticism, interpretation amounting to interpolation—what are they in the main but methods and practices of filling gaps and correcting biases? History has sometimes been severed from the other social sciences by limiting it to the peculiar function of describing the unique occurrence. Some go so far as to assert that this function forever debars it from any attempt at the comparisons, classifications and generalizations which characterize a science. May it not be truer to say that the consciousness of defective material and the consequent hypertrophy of critical talent have tended at times to check the human craving for synthesis? The wary historian, taught and teaching the dangers of historical analogies, has tried to limit his professional risks; but he is increasingly minimizing those risks. In our modern historical studies a rough series of transitional forms may be traced between the two poles of research, between the narrative of the individual sequence, especially characteristic of political history, and the emerging science which essays, by the comparative method, the discovery of the laws or continuities of group action. There must be and must remain the detailed description of the single specimen, the individual sequence. The art of historical narrative will not die. But as the records, with the aid of all the kindred social sciences, become more adequate and their interpretation more penetrating, history, or rather some branches of the craft, may approximate a historical sociology. The interdependence of the sciences is already exhibited in the use which the social historians, as well as the political scientists, are beginning to make of statistics, or at least in the demand for training which will enable them to check quantitatively their descriptive studies.

L. P. Namier's recent book on the structure of English politics at the accession of George III is an example of this use of statistics. Despairing of gaining fresh insights from the accustomed sources, such as the gouty *bon mots* of Horace Walpole's letters or the acrid outcries against the nabobs or the mutual diatribes of parties against the corruption practiced by opponents, Mr. Namier undertook the compilation of a card index containing the fullest details, especially from local records, family and local correspondence, concerning every member of the House of Commons from 1760 to 1783. After years of this minute inquiry, he has published this notable initial study. His text is shot through with numerical tests of such problems as the extent of aristocratic influence upon the representative system, or how much voting power was exerted by the new wealthy class, or the weight of the crown and administration in elections or the supposedly prevalent corruption of the electorate.

As each generation thus rewrites its history of the past—a proceeding necessary because of defective records and because of the shifts in selective interest and in the configuration of contemporary biases—some of the gaps in the record become partially filled. But no amount of research or tenable conjecture can eke out the most serious silences of our sources. This is particularly true in certain fields of economic history. Such institutional structures as the open field system, the craft guilds or the factory, which have left behind them a record of customary regulations or external legislation, have, like the skeletal forms of zoological species, deposited their fossils in the historical strata. But the more dynamic elements, like the vital organs and the nervous system, have tended to disappear. We can with considerable accuracy reconstruct these economic institutions in what we may term their skeletal form; but how and why they came into being, what animating forces actuated them in their prime and gradually ebbed in their decline, are questions not easy to answer from our materials.

Take, for a simple instance, the flow of trade as a formative influence on industrial institutions. Of foreign commerce, its agents and carriers and its extent, we have, for the pre-statistical period, some fragments of information, but for local or domestic trade the material is almost hopelessly inadequate. Very difficult and elusive is also the study of past changes in the standards of living. The record of economic history is therefore unevenly weighted, tempting us to stress successive stages of production and to neglect the dynamic factors of distribution and consumption.[1]

Even our current statistical record is notably weak on domestic trade, and the analysis, statistical and other, of the assumptions underlying the demand curve is inadequate. We shall have in 1930 the first national enumeration of the agencies and amounts of market distribution, and this will be warmly welcomed by economists for the help it may give on this much-needed point.

But why should a busy world produce statistics for the delight of economists? We are told that economists are a voracious species, greedy for figures, only to be satisfied when census-takers and social psychologists stand beside the remaining third of the working population. By implication, we should be satisfied with the existing notable increase in our equipment for economic and social measurement since the World War. Nearly all the departments of the federal government are busy turning out statistical tables in growing volume, but as yet too little coordinated.

Numerous state, regional and local agencies are doing likewise. Trade organizations, university research bureaus, independent institutes, private investigators fill the country with questionnaires and statistical publications. And still further plans for quantitative research, nation-wide and local, are constantly emerging. Is not all this more than enough? asks the business man. The economist answers with an emphatic negative. Our modern world stands but at the threshold of the "statistical period" for which the meager but gradu-

[1] Thorold Rogers had a naïve habit of treating his hypotheses as facts. Hewitt's study of medieval Cheshire records has recently shown up this foible. Rogers first expressed doubt regarding Cheshire's medieval salt production, a commodity of prime importance, and thereupon he converted the doubt into a flat denial. This bald assertion was apparently based on the absence of Cheshire salt from the record of price quotations. But Hewitt's researches (*cf.* H. J. Hewitt, "Medieval Cheshire," 1929, pp. 108–112) make it evident that the Cheshire brine springs were continuously worked and that, though the data in regard to distribution are very scanty, the number of recorded salthouses far exceeded the needs of local consumption. In other words, the strata of the thirteenth and fourteenth centuries show plenty of skeletal forms of production, but the evidence as to the considerable trade which created them was never recorded or has almost vanished.

ally accumulating statistical records of the pre-war century formed merely the initial and preparatory stage. We are far from the saturation point of supply, and the demand is unappeased.

This quite modern craving for exact knowledge of economic and social institutions and processes is a definite symptom of the advent of a new historical era, which was ushered in toward the last quarter of the nineteenth century by a definite reaction against the régime of unfettered, individualistic, secretive competition. Urgent social need, felt a century earlier in western Europe and in the regions within its radiating influence, had unleashed the individual, long straining for free activity, and this release in turn had stimulated the demand for ever more intense production. The world witnessed an unrivaled outburst of industrial energy, an unprecedented movement of migration, an age of unexampled magnitudes and speeds. The spurt of initiative was assisted by a new theory and a resulting policy, in law and economics—rationalistic, hedonistic, individualistic and dogmatic—which, with the clear-cut certitude of a new faith, enforced the destructive but energizing work of liberalism. But, unfolding with an inner logic of growth, out of the older order emerged fresh forces and institutions. Protectionism, with tariffs facing outward and dikes of combination crossing inwardly, restrictionism in its various aspects, including its check on the free flow of immigration, and nationalism in intensified forms—with interculded social cooperative organizations —were among the manifestations of the new spirit. But meantime, with the growing complexity of the social situation, new concepts of relativity, of evolutionary adaptation and of social solidarity were steadily undermining the former certainties of an automatic, mechanical, predestined harmony. The altered pattern of men's thinking, even though, or perhaps because, it was accompanied by a sense of fluidity, has reinforced the dominant demand for stability and social controls.

The need for wisely adapted and therefore more effective controls underlies the craving for more statistical knowledge. If we are to have better social controls, we must have records, many more of them, and more men trained to interpret and to use them. The historians, especially the economic historians, have a peculiar reason for their intense interest in this momentous process of transition going on in our own day. A study of operating social forces in a period of change will yield not strict parallels, but insights and understandings of those earlier operations which have left so impermanent a record. One of the many interesting phenomena which mark such a period as ours is the realignment of public opinion. Who could have foretold in 1890 the change in sentiment which made possible the consolidation provisions of the transportation act of 1920? Yet we can find repeated instances in the past of such almost unconscious refigurement of the public mind, as, for example, the change in opinion in England on the inclosure movement between the sixteenth and the eighteenth centuries. At times, the great changes are initiated and carried on almost unperceived by those who are themselves making them. Common use and wont is one of the greatest enemies of the historical record, for ordinarily those gradual, day-by-day changes in attitude of mind in which multitudes participate are not observed and recorded. New concepts are diffused and slip noiselessly into men's minds; they nestle down beside older alien concepts, often without noticeable jostling; new practices resulting therefrom silently emerge or old ones quietly drop away, until at some point, the preparation becoming far advanced, the new face of the world becomes manifest with apparent sudden-

ness, and the historian, casting about even to set a date to this revolution, discovers another gap in the historical record.

There is strong reason, therefore, at this particular juncture not only to study attentively those hidden things which lie upon the surface of the present, but to collect and preserve the commonplace documents of the immediate past. Already the Business History Society is concerned with the collection, preservation and utilization of business documents, not merely by its Boston headquarters, but by local historical societies generally and especially by university and business school libraries. Such documents as account books, letter files, reports and other records of individual concerns, corporations and trade associations, above all the long-run series which for a time have been heaped in storerooms and then destroyed, should now be preserved, sorted and organized for study. Many of them may be had for the asking, some under pledge of confidence for a certain number of years. What would we not now give for a large accumulation of such material, say from Antwerp of the sixteenth century, or from Lancashire of the eighteenth, to add to such few documents as we possess! Yet we still permit similar records, precious at least to our successors, to be destroyed by hundreds of tons daily.

Towards meeting our own needs and answering our own questions we can do something, and while we can not imagine what questions our successors will put to our time, nevertheless we must build for them as well as for ourselves. It is tempting at times to meditate upon our successors. Perhaps, two or three centuries hence, the highly trained economists in what may at that date correspond to the domestic commerce division of the Bureau of Foreign and Domestic Commerce, at Washington, may be preparing, from the great flow of current information coming daily from all parts of the country, that perpetual inventory of consumable resources, that delicately adjusted economic indicator which will promptly measure and thus help to regulate the fluctuations of business and government enterprise. They may, in their historical section, be reporting on such imperfect but hopeful beginnings as we are making in our statistical laboratories, our surveys of economic and social changes and our presidential conferences. But we can not imagine what kind of ordered world these our successors will be living in, or how far in the rhythm of history the existing tendency to control and stability may be carried. Will they still be discovering new merits in the medieval ideals of the just price and of equality of social opportunity? Or will a reaction have set in against an overdevelopment of the planned national economy and its neo-cameralistic economics?

However uncertain the future, of one thing we may now be fairly sure: as the tides of world history are now moving, each ebb and flow enlarges progressively the knowledge obtainable from the human record. From the imperfections of the social sciences will ultimately emerge more perfect guides to social controls.

# MATHEMATICS AND STATISTICS

By Dr. EDWIN B. WILSON

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It is, I believe, the custom for the president, avoiding technicalities in his address, to give the association some general point of view upon that particular branch of theoretical or applied statistics upon which he may consider him-

self reasonably competent to advise. It has seemed to me well to discuss the relation of mathematics to statistics, partly because so many persons behave as though a great deal of mathematical background were essential to a safe and satisfactory practice of statistics and partly because so many others behave in the directly contrary way as though no mathematics at all were necessary and much were harmful. In treating a topic on which behavior is so varied and opinions presumably even more at variance, it must be apparent that I am taking my life in my hands and that if I survive at all it can only be with fewer friends and more enemies.

Mathematics may be either right or wrong. By this I mean that when you add 2 and 5 together you may get 7, which is right, or something else such as $-3$, which is wrong.

Or, to take a more complicated illustration, if you decide to fit some empirical formula or curve to a set of coordinate data, such as prices and times or populations and times, you may proceed in several ways. First, you may plot the variables and draw in the curve according to your esthetic sense. A curve so fitted can not be either right or wrong, but only a matter of taste. There is no way in which the work can be checked. Of course a dozen persons may be given the same sequence of points and be required each to fit the curve according to his taste; the results of those different fittings may then be compared to determine how much and in what way the solutions differ. I am not averse to this esthetic procedure. When the curve to be fitted is a straight line, it has been found by experiment that the solutions obtained do not, on the average, depart from the "least squares" solution by more than two or three times the probable error of the least squares solution, provided the drawing be made on an adequate scale. In cases where it is not important to check the work and where the precise least squares solution is unnecessary, the graphical method is often the best because the easiest to follow.

There are persons, however, who like to go through the work or force their assistants through the work of a least squares solution even when it is not necessary. And of course there are many cases in which to avoid the work of a least squares solution would be to shirk. One great advantage of such a solution is that it can be checked; it is either right or wrong. Every person who does the work correctly, starting with the same set of data, with the same weights assigned to each point and with the same empirical formula to fit, should get the same answer to that degree of arithmetic accuracy which is justified by the number of places carried in the calculation and without any regard to whether the original data justify carrying so many arithmetic places or not. It is a matter of taste whether one determines to apply least squares; it is also a matter of taste whether he assigns one set of weights or another to the various points, and it is often a matter of taste whether he selects one or another type of empirical formula to be fitted; but once this is all determined, the answer is right or wrong just as $2+5=7$ is right and $2+5=-3$ is wrong—or better, as $\sqrt{2}=1.414$ is right (to four places) and $\sqrt{2}=1.305$ is wrong (even to two places).

Although mathematics may be right or wrong, I believe it is fair to claim that it should be right. It is difficult to undertake to prove that mathematics should be right. We are here dealing with a question of ethics, not with one of science. We do not expect an artist to be right or wrong; we could hardly accuse a metaphysician of being either right or wrong. The criteria of excellence of

performance in all such cases must be based on tastes—the tastes of the performers and their critics, and, according to the old maxim, *de gustibus non est disputandum*, albeit I know of nothing about which there is so much dispute as tastes, and for obvious reasons, which may account for the necessity of the maxim. The reason that mathematics should be right is because it can be; and for this reason, to avoid disputes, it has become the professional ethics of those who apply mathematics to get that part of their work right. So wide-spread is this ethics that in practice one rarely tries to check the purely mathematical work of an investigator. It is not that the principle of corroboration which pertains to all scientific investigation is in abeyance, but merely that the mathematician is expected to check, *i.e.*, to corroborate his own work so that the reader may take it on faith. Let us borrow the terminology of our societologists and say that it is in the folk-ways and mores of mathematicians that their mathematics should be right. Let us treat it as an axiom.

Why, then, should I make so much of the matter? Precisely because there are so many persons now using mathematics for the first generation in their respective sciences that it is advisable to set out what is the professional ethics of applied mathematicians. And because I regret to say that I have come across work by well-known investigators, in famous institutions, financed by generous foundations, work printed in journals of international repute, but unhappily so far from right that its wrongness can be recognized at a glance by any one really familiar with mathematical procedure. Evidently the work could not have been checked by the authors as it should have been or really read by an intelligent editor. This kind of writing and editing is a real imposition on the reader and necessitates some emphasis upon the duty of him who uses mathematics to get it right. Of course, we must not be too severe; accidents will happen and in any particular case it comes down to a matter of judgment as to whether negligence has been criminal.

Mathematics, assumed now to be right, may be appropriate or inappropriate to the problem in hand. This appropriateness is again very largely a matter of taste. One great advantage to the investigator of being familiar with a considerable variety of techniques and of the mathematical background of those techniques is that he has a greater experience and a sounder knowledge as a basis for his judgment as to appropriateness and is therefore likely to have better taste in such matters. I made mention of the method of least squares. I wonder if many of you realize how pervasively that method penetrates our statistical procedures even when it does not appear on the surface. Suppose we add a column of figures and find the mean. Do we stop to think that the mean is the least squares solution of the problem of finding one item to represent the whole group? The mean is nearest to all the elements of the group only if we take as the criterion of nearness the squares of the eccentric departure and not the departure itself. It is the median which we should use if our notion of nearness is to be founded on the departure instead of on its square, and I may recall to your mind that the great economist and statistician, F. Y. Edgworth, had a considerable partiality for medians—I know not why, perhaps merely as a matter of personal taste. Fortunately for many if not most problems, conclusions based on working with medians or with means are for practical purposes identical, but when they differ one may well hesitate.

To discuss a little this question of appropriateness of mathematics, let us take the purely hypothetical question: Where

should our association meet? How might a scientific member from among us solve this problem? As to this I have no idea; the ways of scientists are often inscrutable. One type of mind would doubtless construct a spot map of our membership scattered as it is about the country. If very conscientious, he would recall that many who are not our members but belong in associations which use statistics should perhaps be given some consideration, and he might therefore add to his spot map with somewhat reduced weight points to represent them. But how then select our meeting place? Should he determine the center of gravity of all the centers of gravity of all these points? Some would do so automatically. They would be, in fact, maintaining that the best point to meet would be that which minimizes the sum of the squares of the railroad fares that would be paid by all persons if all attended. And others might reason that what we needed was the point of greatest concentration of our population on the ground that it would be of maximum convenience to the greatest number—a just procedure provided not many would come from a distance anyhow. This would be selecting the mode. Still others might feel that all interests were best met by that point which would make the sum of the railroad fares least, being in that median sense the point nearest to the whole membership and thus presumably most convenient on the whole. It is clear that the scientific solution could be obtained only after determining that type of mathematical formulation which was most appropriate to the problem, and this determination would be a matter of taste.

Fortunately, we all know that we may be spared such a scientific determination of the place where we shall meet. Like as not, nobody would be satisfied with the solution anyway and at all events there would be years of dispute over the appropriateness of any methodology. Much more practical means suffice. The Economic Association determines willy-nilly to come to Washington; the Political Scientists feel an irresistible urge to celebrate their twenty-fifth anniversary at New Orleans, and the rest of us decide where we will go only after much discussion and perchance with small rational basis. This is one of those cases where pretty much all mathematics is inappropriate, where any mathematics whatsoever will give an entirely false sense of precision to a problem in which no precision inheres. And there are many problems in economics or sociology or the public health which are as yet very much in the same position and where we need not so much some kind of mathematics or some particular statistical technique as a general survey of a wide range of facts, many of them qualitative, which may serve as a basis for some decision.

Probably the majority of problems to which the statistician must turn his attention are in reality somewhat intermediate between those in which the technique to be applied is clear and those in which no technique, at least of a mathematical sort, is advisable. In this methodological no-man's land, the statistician must do the best he can. He may have to develop a new technique; in that case he has two chief reliances, first and foremost a sound and wide acquaintance with the field of activity in which he has to operate, and second a good mathematical background, because it is from that that all techniques are developed; but if choice must be made between familiarity with his subject and familiarity with mathematics, I should unhesitatingly prefer the former. Mathematics is a queer horse and all too easily runs away with its rider; and then there is such a satisfaction in trying its various gaits in all sorts of roads that many a rider has gone off in almost the opposite direction from the path he should have followed

in his pursuit of the solution to some scientific problem; he may have ridden right over his solution to some purely fantastic goal.

Each person has to do the best he can to bring to bear upon his work the talents and the training that he has. If I may be pardoned a personal confession, I will say that I have never been sorry that in my youth I acquired an unusually good acquaintance with mathematics. At times when engaged upon some statistical problem in physics, I have needed to learn new mathematics, but for the most part my early training has sufficed not only to let me follow the mathematics of others but, so to speak, to see through it with an assurance which sometimes convinced me that it was hollow. And if in my present studies I use but little of the vast amount I have once learned, it is not because I do not like the exercise of using it but because I prefer tools more appropriate to my job even though not so refined—one gets ahead faster.

Mathematics may be right or wrong, it may be appropriate or inappropriate, it may be useful or useless. There is many a useless problem. Sometimes one can not see it is useless until much time has been spent upon it. But consider this: I have a field in New Hampshire much covered with stones, rocks and boulders. With sufficient energy I could weigh all those objects, divide the weights into suitable intervals, plot a histogram and fit a frequency function. That would be scientific observation, followed by mathematical treatment of the observations. The problem is obviously statistical. It would keep me occupied for some time. But what would be the use? Of course if I knew enough about geology to be confident that my field was a fair sample of a large group of fields and if there was any geological or agricultural or constructional or other interest to be served in determining a frequency function of the weight of superficial stones from small to large from a "sample" field the job might not be useless but useful. But there seems at present no good purpose to be served by its doing. And often I wonder in some of my reading whether the hard work the author has done, though correct and appropriate in its mathematics, may not be quite useless.

What we need to foster is useful, appropriate, correct mathematics applied to worth-while scientific problems, and worth-while scientific problems whether or not they have reached the stage where any considerable use of mathematics is helpful. And in these vital matters, we have far more need of good taste and a sure instinct than is commonly believed, for it is so often only in the future that we can get a statistical estimate of the worth-whileness of present activities. Happy is the statistical investigator who can use all his techniques with discretion, and happy the teacher who can give his students some sense of proportion as well as a group of methods in such a conglomerate field as statistics.

Placed as I was upon the program between sociology and economics, represented by such giants as Presidents Ogburn and Gay, interested the one in making himself a mental centrifuge to precipitate the facts of recent social change from that murky colloidal solution we call the present, the other bent on crystallizing out from the subcooled liquor of our past the solid course of prices, it was quite impossible for me to determine whether I was to be the meat of your sandwich or the comic relief of your tragedy. Without pausing now to suggest an answer to this question, but rather in the continuing line of my previous remarks, may I direct your attention to that interesting statistical treatise "The Bridge of San Luis Rey," by Thornton Wilder. Some of you recall the story. The bridge fell, killing five

persons, and the devout Brother Juniper was struck by the question: Why did this happen to those five? And determined to surprise the reason of their taking off. It seemed to him that it was high time for theology to take its place among the exact sciences and he had long intended putting it there. What he had lacked was an opportunity for observation under *proper control.* Previous happenings had been involved, but here was a sheer Act of God and at last His intentions could be studied in a pure state. It was not, however, Brother Juniper's first essay in scientifically examining the ways of God to man. There had been a deadly pestilence of smallpox in his village and he had recently drawn up a diagram of the characteristics of fifteen victims and fifteen survivors, the statistics of their value *sub specie aeternitatis.* Each soul was rated upon a basis of ten as regards its goodness, its piety and its usefulness to its family group. As follows:

| | Goodness | Piety | Usefulness |
|---|---|---|---|
| Alfonso G. | 4 | 4 | 10 |
| Niña | 2 | 5 | 10 |
| Manuel B. | 10 | 10 | 0 |
| Alfonso V. | –8 | –10 | 10 |
| Vera N. | 0 | 10 | 10 |

The investigation developed difficulties; almost every soul in this little community turned out to be economically indispensable, and the column headed "Usefulness" was all but useless; negative signs had to be introduced to distinguish from the good and bad those who were not only bad (Grade 0) in and of themselves but actively led others into wickedness. From all this data the good scientific friar contrived an index for each person. He added up the total for the victims and compared it with the total for survivors—to discover that the dead were five times more worth saving. And then, taking a walk by the Pacific he tore up his findings and cast them into the ocean—a most profitable mode of publication and in a medium of the widest circulation.

So when he came to study his one great chance, the collapse of the bridge with its five victims, having experienced the bitter disappointments of statistical procedure, Brother Juniper forsook the method of W. F. Ogburn for that of W. I. Thomas—the case study. In compiling his book on th se five victims he omitted no slightest detail for fear he might lose some guiding hint. He put everything down in the hope that the countless facts would suddenly start to move, to assemble and to betray their secret. Of one, the Marquesa de Montemayor, he learned from her cook that she had lived almost entirely on rice, fish and a little fruit, and he put it down on the chance that it would some day reveal a spiritual trait to aid in sifting the inscrutable ways of God. From another, he learned that she came unbidden to his receptions to steal the spoons. A bookseller testified that she was one of the three most cultivated persons in town. The midwife declared that she had called upon her with morbid questions until she became a nuisance; a servant that though absent-minded she was a compact of goodness. And so with many another in her respect and in that of the other four. We may pass over the conclusions to state that the book being finished was pronounced heretical and our scientific theologist Brother Juniper was burned alive with it—apparently to the great regret of everybody, but the simple persons of his time may not have had our overburdening experience with questionnairing.

With an apology to you and to Mr. Wilder for these few free quotations from "The Bridge of San Luis Rey" couched in a crude English that does him no justice, I will leave you with the suggestion that you study further this remarkable statistical romance, and I will

venture the hope that should the author make a few dollars extra from royalties because of this hint of mine, he should apply them to securing a membership in our association. Some of us need him in our business.

# THE FOLK-WAYS OF A SCIENTIFIC SOCIOLOGY

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For long it has been a practice of sociologists to study the habits and manners of peoples. Therefore, it should be permissible to examine the habits and manners of sociologists, if the subject were sufficiently significant. It becomes significant, perhaps, and certainly intriguing, when one thinks not of the habits of present-day sociology but rather of the practices of a sociology of the future, when it has become more truly scientific than is the case to-day. Naturally we can not make precise predictions, but certain inferences can be drawn; and, then, it should be remembered that one of the customs of long standing among us is that a president of a scientific society in his presidential address is not expected to be bound so rigidly by the restrictions of data, nor is his imagination to be so disciplined as would be the case if he were presenting the results of a piece of scientific research. I shall draw rather more largely than usual on this freedom which I understand is allowed me.

One of the processes that will shape the sociology of the future is that of differentiation, described at such length by Herbert Spencer. It is, I know, not the fashion to quote Spencer these days. But irrespective of our intellectual styles, division of labor and differentiation continue to be powerful processes despite frequent exceptions. Historically the growth of the sciences has occurred by a processs of differentiation. Indeed, it is customary to note that philosophy was the mother of the sciences and that sociology and psychology are the latest to be separated from her, if, indeed, the separation may be said to have been completed.

No doubt this differentiation of subject-matter will continue, yet the reverse process has undoubtedly been operative, particularly in the United States, during the past decade. The reason is that this differentiating process has reached a stage where a particular science is quite inadequate to deal in any realistic manner with many practical problems, any one of which falls in the several different fields of the various social sciences, so that the dividing lines between the social sciences have been breaking down under the impact of certain researches particularly in the practical world of social life and the many problems which it presents.

But it is another differentiating process which will be of special significance for the future of sociology, a differentiation not so much of subject-matter as of methods. In short, the more strictly scientific methods will be differentiated from methods that more properly belong to activities other than those of science. I refer to such activities as are found, for instance, in ethics, religion, commerce, education, journalism, literature and propaganda. Sociology as a science is not interested in making the world a better place in which to live, in encouraging beliefs, in spreading information, in dispensing news, in setting forth im-

pressions of life, in leading the multitudes or in guiding the ship of state. Science is interested directly in one thing only, to wit, discovering new knowledge. As a human being, I, of course, want to seek for knowledge that will be of benefit to mankind. Similarly as a human being, I may want to spread this new knowledge far and wide, or to affect the beliefs of people or to write my interpretations of life and events. These activities may be just as important, or more so, perhaps, than discovering new knowledge. Indeed, it is hard to rank them in values, since they are all invaluable. But in so far as I function in these respects, however worthy they may be, I am not engaged in scientific activities.

The differentiating process will split off these various non-scientific procedures that are now so intertwined in the so-called scientific pursuits of the social scientists. When this is done, the sociologists will have abandoned some of their existing habits and will have developed some new ones instead.

One of these new habits will be the writing of wholly colorless articles, and the abandonment of the present habit of trying to make the results of science into literature, the precedents set in this regard by Huxley and William James being considered a bad legacy for the apprentices of science. It will not be necessary, then, to end articles with an eloquent appeal or a scintillating conclusion. It will be possible also to begin articles without referring to Plato, Aristotle or any other of the much-praised Greeks. Clarity and accuracy will be the only virtues of exposition. The expression of emotion will be bad form. The audience for these articles will be the scientific guild, and no attempt will be made to make these articles readable for shop girls or for the high-school youth. Articles will always be accompanied by the supporting data. Hence the text will be shorter and the tables and records longer. It will cost more to print them. Only part of the article will be read; the remainder will be for reference.

This specialization in the exposition of science does not mean that there will be any diminution in the popularization of science. There will be numerous articles and books which will show the human significance of these discoveries and measurements—publications which will dramatize science, which will rewrite scientific results in terms of slang, which will put in them an ethical punch. The scientist himself may engage in such types of writing, but if so it will be in the capacity of another self, not as the functioning of his scientific self.

And so there will be a new type of social science journal, not now in existence, save perhaps in one instance, which will devote itself to the publication of scientific results for a scientific audience. The articles in the new social science journals will be in some ways greatly expanded social science abstracts, that is, an abstract in the sense that the scientific essentials will be abstracted from the irrelevant interpretation, popularization and emotionalism. And so readers will go to the sociological journals in the future for one thing only, to find new knowledge. They will not expect, as they do now, a gratification of their esthetic sense, ethical edification or entertainment or stimuli for the projection of their personality.

In the future era of scientific sociology there will be a marked decline in the prestige of intellectuality as such, as compared with its vogue in the nineteenth and twentieth centuries. But this decline in the prestige of intellectuality will be only among the scientists themselves, for the difference between scientific activities and intellectual activities will be more sharply drawn. All scientists are intellectual, of course, but

only a very few intellectuals are scientists. The difference is very well noted in the comparison of two addresses by Renan and by Pasteur, at the time of Pasteur's reception into the Académie Française. Renan's was a great intellect, which shone in full brilliance in his address of welcome, an address of wit and charm, of lights and shades, abounding in intellectual subtleties, scholarly references and touching at times the profound. Pasteur, the scientist, in his address of acceptance, was not at home in the display of intellect as such. His was a simple, straightforward and by comparison dull presentation. This does not mean, of course, that scientists may not be great intellects. Quite the contrary. Intellectual play or display may be the recreation of the future social scientist, but hardly his main work. The disciplining of the mental processes is too strict in scientific work to permit intellectualism to flourish in the laboratory. Intellectual processes—as contrasted with scientific thought—are combined usually with feelings, though of course always in logical form. Impressions are followed more freely in intellectual life, wherever associations lead. But in the scientific work of proof, of establishing real enduring knowledge, thinking must be freed from the bias of emotion. There must be eliminated all the associations that disturb the closeness of the connection between the thinking and the data.

Of course the disciplining of thought is not so apparent in one of the steps in scientific work, *viz.*, the originating of ideas, or in the slang equivalent, "the getting of hunches." There imagination and free association are the greatest aid to the scientist. It is for this reason that one says, and quite truly, but rather crudely, that there is something of the artist in every great scientist. So intellectualism is the proper atmosphere for the birth of ideas. Getting the idea is often said to be the first step in the scientific process, but more often it does not lead to the new step—it only leads to the production of literature. But it must be remembered that getting an idea is not establishing real knowledge. An idea of value to science must be formulated in some sort of form capable of demonstration or proof; then must follow the proof or verification. An unrestricted propagation of ideas will not produce science. The shaman or medicine-man of the American Indians was not a scientist and did not produce scientific medicine, although he was very fertile in the production of ideas. The originating of ideas is a necessary step in the scientific work, but ideas must be formulated and tested by reality.

With the decline in intellectualism it will be less easy to get fame as a theorist, and with the rise of science reputations will be built upon proofs, records and measurement. But at times, fortunately gone by, in some of our social disciplines, a man had rather have been called a theorist than a scientist, a most peculiar twist in values, for in the natural sciences it is rather a matter of shame to be labeled as only having set forth a theory. The publication of guesses, hypotheses or hunches in this future era will be tabu. There will be no virtue in a merely stimulating article. The *sine qua non* of scientific publication will be verification and evidence. Verification in this future state of scientific sociology will amount almost to a fetish. There will inevitably be a great many unimportant and uninteresting things verified. Thus science will utilize the dull and uninteresting person, just as logic utilizes the paranoiac, as social philosophy utilizes the fanatic and as intellectualism utilizes the day-dreamer. For science will rest on a base of a great deal of long, careful, painstaking work. And many stupid persons can be careful, patient, methodical.

It must always be remembered that science grows by accretion, by the accumulation of little bits and pieces of new knowledge. Occasionally one of these little pieces of new knowledge becomes of very great significance and it is then called a great discovery or a great invention. But one can not predict when these very significant pieces of new knowledge will be discovered. The accumulative nature of the growth of science is usually not appreciated because of the prevailing opinion, influenced in part by the Comtean postulates, that the stage of proof and verification in the development of science must be preceded by a long period of theory. Such a sequence is true enough in the case of the verification of a particular hypothesis but not necessarily so of the development of a science. The growth of science is rather the accumulation bit by bit of new and lasting knowledge. The accumulation of these new discoveries calls for an organization of workers, not all of whom will be stupid, however, for brains are useful in science as in every other form of enterprise, and there will be geniuses in science as truly as in scholarship.

In this future state every one will be a statistician, that is, nearly every one. All the universities will have statistical laboratories and the individual workers will have plenty of machines, all of them electric. Indeed, there are likely to be more machines than thinkers. For some time, perhaps a very long time, however, a goodly portion of research in sociology will make no use of statistics. It is obvious, however, that quantitative sociology is bound to have an enormous growth, not only because of its undoubtedly great usefulness but also because we have the wealth to collect the statistics and the organization to provide for their analyses. While all sociologists will be statisticians, statistics as a recognized field of knowledge will disappear, and there will be no professors of statistics. Statistics will disappear as a distinct field of knowledge because it will be almost universal, not only in sociology and in economics, but perhaps in social psychology and in political science also. All the journals in the different social sciences will publish statistical articles. With the growth of statistical research, it will become more and more apparent that statistical method as such can not well be divorced from the data. Hence, statistics will be identified with the subject-matter in each social science rather than be set apart as a special discipline. Indeed, this tendency is making great headway in the United States at the present time—much more so than in Europe.

In the past the great names in sociology have been social theorists and social philosophers. But this will not be the case in the future. For social theory and social philosophy will decline, that is, in the field of scientific sociology. Social theory will have no place in a scientific sociology, for it is not built upon sufficient data. Of course, certain syntheses of broad researches may be called theory, a new meaning for an old term. But such syntheses will be based on evidence. Social theory in good part is a product of wishful thinking, taking form in the *Zeitgeist* in which it is developed. But so were the superstitions of the primitive cultures, such as, for instance, the theories as to the origins of the world and how it was peopled. Many of the great social theories will collapse, just as the theory of supernatural beings once collapsed, for it is possible for a great body of intellectual ideas of a people to have no abiding truth in them. A scientific sociology will be quite sharply separated from social philosophy, for it will be recognized how much social philosophy is a rationalization of wishes. Social philosophers will continue to exist, however, and serve a

very useful purpose in such fields as ethics and among publicists and statesmen.

One of the qualities most sought after by scientific sociologists will be patience, which will be accorded rank as one of the major virtues. Fame, publicity and emotional gratification will appear always as temptresses, but the scientist will be loyal to patience. Although the dynamic qualities of human nature are furnishing him with the drive to make his goal, nevertheless, without caution and suspended judgment, they will prevent him from attaining that goal. Brilliance and originality will always be admired, but in a scientific sociology they will never be admired alone, that is, without the accompanying proof and measurement that comes with perseverance and patience.

This insistence upon "suspended judgment" is not compatible with action, which tends to follow directly out of emotion. The caution of the scientific mind in reaching a decision is illustrated by the conversation of the scientist and his traveling companion as they looked out of the train window at a herd of sheep. To the remark that the sheep had been sheared, the scientist replied, "They seem to be on this side." Such extreme caution and insistence on "suspended judgment" is not compatible with executive ability. It is, though, a desirable quality in judicial ability, although judicial decision is more often hasty than is scientific conclusion.

The scientific sociologists will not, therefore, be statesmen, leaders or executives. And if they ever seem to guide the course of evolution—which neither they nor any one else ever can do—it will be indirectly, by furnishing the information necessary for such supreme direction to some sterling executive who will appear to do the actual guiding. In some rare cases a person may be both a scientist and an artist. But, if so, the guiding of the ship of state will be done by only one of his two personalities, the executive one. This differentiation of the scientist from the administrator has already gone far in this country, farther than in Europe, and it is noticeable in our universities.

While the sociologist as scientist will not hold office or lead movements, this does not mean that he will be an armchair sociologist or that he will necessarily live a secluded life. On the contrary, the scientific sociologist must become more and more realistic and he must learn to know his data by the closest of connections with the sources wherever they may be, in social movements or amidst social problems. He will be found with the staff of the courts, in the factory, at the headquarters of the political party, in the community centers. He will be wherever data on significant social problems are to be found. But he will be there as a student to discover new knowledge and relationships rather than as a practical worker. The executive, the leader, the social worker will be the group to put to use the information which the scientific sociologist furnishes. For, as some wag has said, "Making butter is different from spreading it."

In the future the subject-matter of the social worker and of the sociologist will be the same, in large part, except that the field of the sociologist will be larger and will encompass that of the social worker. The interests of the social worker and of the sociologist will also have more in common, for a large group of sociologists will deal with the practical problem of human betterment. And to a certain extent the motivation of the social worker and of the sociologist as a human being will tend to be the same, for the social scientist, being human, will be interested in making the world a better place to live in—at least, most of them will. But they will go about it in

different ways. The sociologist will of course work on the problems that tend to make sociology an organized systematic body of knowledge, but also he will choose for his researches the study of those problems the solution of which will benefit the human race and its culture, particularly those problems that present the greatest acuteness. But the scientific sociologist will attack these problems once chosen with the sole idea of discovering new knowledge. Whereas the social worker will be interested in applying the new knowledge thus discovered for the alleviation of the ills of mankind, either as a social engineer or as a leader of a movement or the executive of institutions. But with the rising standard of living which will come with a lowered birth-rate, with the many new inventions that are inevitable and with our wonderful natural resources, the nature of the problems of the social worker will tend not to be set off as a class dealing with poverty but rather with social problems in general. The social worker and some of the sociologists will thus work together at the same place, the one interested in discovery and the other interested in practical achievement. The two functions may indeed find existence in the same person.

Both the sociologist and the social engineer will require much more scientific discipline than do the natural scientist and the mechanical engineer. One reason is the greater temptation to distort conclusions in the interest of emotional values. Furthermore, the social engineer will not be able to restrict himself to the application of proved knowledge, for social problems will be so urgent that one can not wait on the "suspended judgment" of the scientist. Something will have to be done. We must vote on the first Tuesday after the first Monday in November, whether our information is complete or not. Social problems call for action as well as knowledge. Now, knowledge is usually a matter of probability. Hence action will often be based upon approximate knowledge, when, for instance, the probability appears greater than 50–50. Fair success in using approximate knowledge in important issues will of course bring social approval. This means that high standards of science will always be hard to maintain in the social sciences. Also, unless social values change greatly from what they are to-day, the leader and the executive will command greater prestige than the scientist who discovers new knowledge for the leader and executive to use. There will always thus be a great mongering with proximate information. But there will also be social engineers who, like physicians in general, are not scientists, but who apply reliable scientific procedures and relatively exact knowledge. But the scientist's work will be differentiated from that of the handling of proximate information and the applying of exact information already known according to formula.

A great deal of research will be done outside of universities, an increasingly large proportion. A smaller and smaller proportion of research will be done single-handed by the lone researcher. This is regretted by some schizophrenic persons who believe that one can not think if one works in an organization. The fact that a clerk's life is a routine and that the punching of adding machines is mechanical is not evidence that nowhere in the organization are there persons who think. All governments, national, state and city, may be expected to increase their research functions greatly. But so also will trade unions, employers associations, leagues and civic bodies, political parties, industries and social work organizations. Increasing wealth will make such social research possible, and secondly, its effectiveness

will be increasingly demonstrated. All these various organizations with special interests will be doing research with a specific purpose to prove a particular hypothesis or to gain a desired end, but their research staff will be dictated to only in the choice of the problem or hypothesis. They will be free to abide wholly by the evidence. To do this they must be sharply distinguished from the executive or policy-making branch.

This differentiating process which will mark off science in sociology leaves us without a wholly attractive or ideal picture of science and scientists. But a forecaster is not interested in whether what he sees is beautiful or not. His idea is to predict solely what will happen. But of course I realize that according to the folk-ways of America in the first part of the twentieth century, all addresses, like the moving pictures, theaters and short stories, are supposed to have happy endings—particularly presidential addresses.

The happy ending for a scientific sociology will be its achievement. It will be necessary to crush out emotion and to discipline the mind so strongly that the fanciful pleasures of intellectuality will have to be eschewed in the verification process; it will be desirable to tabu our ethics and values (except in choosing problems), and it will be inevitable that we shall have to spend most of our time doing hard, dull, tedious and routine tasks. Still the results will be pure gold and worth the trouble. While science will separate itself from education, propaganda, ethics, journalism, literature, religion and from executive leadership, of course all these excellent social activities will not cease. Social life will be thus just as rich. And, finally, it is not necessary for a scientist to be a scientist all the time. He can temporarily shut the door to his laboratory and open for a while his door to the beauty of the stars, to the romance of life, to the service of his fellow man, to the leadership of the cause, to the applause of his audience or to adventure in the great out-of-doors. But when he returns to his laboratory he will leave these behind, although there is a beauty, a romance, a service, a leadership and an adventure of a kind to be found sometimes in the laboratory.

# LIGHT THROWN BY GENETICS ON EVOLUTION AND DEVELOPMENT

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At the beginning of this century two epoch-making events occurred in biology. One was the publication of de Vries' "Mutationstheorie." This book gave the results of years of experimental breeding with several species of plants, but especially the evening primrose. It opened up a lot of questions that plainly could be answered by the experimental method. It clearly revealed that variation could and must be made the object of experimental inquiry.

The other event was the rediscovery of Mendelism. Mendel's paper of 1865 was brought to light practically simultaneously through the independent work of de Vries, Correns and Tschermak. Their work, confirming Mendel's, proved that heredity could be profitably studied experimentally.

Now mutation and heredity are clearly factors of evolution. The experimental study of mutation and heredity inspired by these writings was begun twenty-five years ago on a large scale by many persons; and we might have hoped, and many of us did hope, that we should come to understand better the factors of evolution; that we should, indeed, be applying the experimental method to the problem of evolution itself.

Not all biologists were friendly to that view. Bateson, especially, insisted that heredity should be studied merely as a branch of physiology; and I am sure that he felt that the name, "Station for Experimental Evolution," which had been applied to a department of the Carnegie Institution of Washington was ill chosen. Bateson alas! is gone now, but the question he proposed is still with us: "Is the experimental study of mutation and heredity capable of throwing light on evolution?" Twenty-five years of endeavor have given experience bearing on the answer, and it is to this question that we will in a minute turn.

At the beginning of the century there was still a third biological subject that had been investigated longer than the other two. It was not commonly, if anywhere, regarded as having any relation to the other two that I have mentioned. This is the study of development of the individual. There is no science that has a more interesting history. It began with the investigation over one hundred and fifty years ago, by Kaspar Friedrich Wolff, of the development of the chick inside of the egg-shell. He was led to make cross sections of the early stages and to note the marvelous foldings of membranes that constitute such an important part of the process by which organs are formed. Then came von Baer, whose studies were so extensive as to lay the foundations for embryology as a new science. The key-note of this science was, for him, *Beobachtung und Reflexion*. For Francis Balfour it became *Beobachtung, Vergleichung und Reflexion*. The last third of the last century will be known in biological history as the period of enormous development of this science. With the aid of new methods of sectioning and staining and with the opportunities afforded by marine laboratories the developmental stages of all classes of animals were being investigated. The first aim of that period was to gather light on interrela-

tionships of organisms; to reconstruct the phylogenetic tree. Ontogeny, it was held, is an epitome of phylogeny.

Later, development became studied as a physiological process; the determination of the spindle-axes in cell-division, the interdependence of parts in development and finally the phenomena of differentiation in development were studied, and the latter, as the subject-matter of causal, or experimental, embryology, is being actively pursued to-day. But in all these phases of study of embryology its contrast to the study of evolution was plain. In general, ontogeny and phylogeny remained very distinct and apparently unrelated disciplines.

At this time I propose to consider the light that experimental work on mutation and heredity has thrown on the process of evolution and to show that embryology is now closely associated with the other two subjects and throws its light on the nature of evolutionary processes.

First of all, it becomes necessary to redefine "heredity." The definition of resemblance between parent and child is quite inadequate and unbiological. I think we may more appropriately speak of heredity as the science of the internal factors that direct development. This definition ties up heredity and development very intimately.

That there are such internal factors is obvious to any student of the development of the egg into the adult. I may place two just fertilized eggs, one of a starfish and one of a sea-urchin, in a finger-bowl of sea water, and they will begin their development in a uniform environment—uniform at least to the outside observer. In the course of a few hours, or days, one of them will develop into a young starfish and the other into a young sea-urchin. The external conditions are the same. The internal driving influences are responsible for the difference. This fact does not oppose the other fact that if I were to place two fertilized eggs of the starfish, one in ordinary sea water and the other in sea water in which the proportion of salts had been doubled, the resulting young starfishes would be very different in form, if indeed both survived. In studies in heredity a uniform optimum set of environmental conditions is assumed. The fact that changing environment may induce changes in the course of development is not denied or overlooked. It is for the purposes of the problem of internal directive factors for the moment disregarded, since environment is made as fit as possible for the developing embryo.

The mechanism of inheritance has long been a subject of speculation. Since egg and sperm are equal carriers of heredity, yet have little in common but their nuclei, the nucleus was early regarded as the carrier of the mechanism of heredity. Weismann worked out in detail its rôle, and on speculative grounds concluded that the germ-plasm or the totality of the chromosomes was the special mechanism. The work of the past quarter of a century, under the influence of Morgan, has placed this hypothesis on a firm basis of fact. Now we know the essential thread in the course of evolution; it is the germ-plasm; and the problem of evolution is the problem of the history of the germ-plasm. Some one has said that the hen is the egg's method of making more eggs. One may even more truly say that the hen and the cock, the man and the woman, are the germ-plasm's method of perpetuating itself; of multiplying infinitely its particular kinds of genes.

We exist as we are because our germ-plasm has determined us, and we fail of our *raison d'être* if, that germ-plasm being evolutionarily fit, it fails to be carried on. Perhaps, one may argue, the failure to be carried on indicates unfit-

ness for evolution. More properly it may signify an unfitness of our woefully distorted civilization to permit of the continuance even of the otherwise excellent germ-plasm.

But if mankind has too little regard for its duty of perpetuating its own best germ-plasm it has no doubt at all of the value of the best germ-plasm of the thoroughbred horse or the dairy cow. Hundreds of thousands of dollars will be paid for a stallion whose racing days are over, but whose personal and family history prove the peculiar value of its genes. By weight the best germ-plasm is the most precious of all purchasable materials. On the other hand, it may be the most precious of free gifts. Little wonder it is so valuable; it has cost an infinitude of lives to make it what it is.

But the continuance of the germ-plasm, though necessary to evolution, will not suffice to produce any evolution. Evolution implies change. Let us see what the experimental work of the past quarter of a century has brought to light in regard to changes in the germ-plasm—which we call mutation. It is in this field that the greatest discoveries have been achieved. This is the great contribution that the twentieth century has already made to our knowledge of evolution.

The quarter century of experimentation has shown us the way to separate inheritable variations, or mutations which have an evolutionary significance, from paravariations which are primarily somatic in their nature. But the clear distinction between these two types of variation has with further study become vague, inasmuch as even paravariations are usually produced under the influence and control of the internal factors, operating always in relation to environment. The effect of environmental factors depends not merely upon their nature, but above all upon the nature of the genes also.

The experimental study of mutation has shown us, first of all, that the chromosomal mechanism, so carefully protected, so carefully evolved and so well fitted for its purpose of continuing the organic world, is subject, by its very complexity, to accidents.

First of all, we are struck by the marvelous behavior of chromosomes among themselves. As is well known, the somatic chromosomes of an individual, A, are ordinarily in pairs, one of each pair having come into A with the egg and the other with the sperm. At the time of formation of the gametes—at meiosis—these pairs of chromosomes come together and lie alongside of each other, like ends together. If the chromosomes of the pair differ by only a few genes they may still undergo this synapsis which precedes gametic formation. But if the corresponding chromosomes are very unlike, as is the case when the egg and the sperm that united to form the zygote A are very different (of different species M and N), the corresponding chromosomes may not undergo synapsis when the germ-cells are formed. The germ-cells may contain a heterogeneous collection of chromosomes unreduced in number in which certain essential chromosomes are missing, while others are double in number. Such zygotes die. The species cross is sterile.

But if, by chance, any germ-cells receive a complete set of the chromosomes of both M and N and two such germ-cells come together, then the hybrid will have each chromosomal element of both species (M and N) doubly represented and the hybrid will propagate itself indefinitely with the double set of chromosomes, as Karpetschenko and others have shown. Thus will have originated, by hybridization, a new form which breeds true, has several or many new

differential characters and is practically sterile with other forms. It has all the characteristics of a new species; it is a new species.

That in nature new species have sometimes arisen in this fashion is indicated by the marvelous fact of polyploid series which have been worked out in some plants. Thus there are found in nature allied species whose chromosome numbers are multiples of a basal number. For example, in wheats we get chromosomal numbers 7, 14, 28, 42, being multiples of 7. In the genus Rosa are found 14, 21, 28, 35, 42 and 54, all but the last being multiples of 7. In Rumex is one series 8, 16, 24, 32, 48, 64, 80; another series, 12, 24, and still another series 20, 40, 60, 80, 100 and 200 chromosomes. In Datura 12, 24, 36 chromosomes and other multiples of 12 have been found in the course of experimentation. Polyploidy has clearly played a part in species formation.

Again, chromosomes undergo other changes which induce great variability and even variety-formation. The variability of the evening primrose, which attracted de Vries' attention, is now recognized as due to varied chromosomal arrangements. In Datura the phenomenon of cross unions between nonhomologous chromosomes discovered by Belling has been further investigated by Blakeslee. Some of such unions may well give rise to constant new species.

As to the cause of these chromosomal mutations some things have been learned. Thus different types of radiations will induce them. It is probable that cold, by reducing the protoplasmic sensitivity, is an important agent. Now polyploid species grow more vigorously than those with fewer chromosomes; cold induces polyploidy and since it is useful for the plant to be able to grow as rapidly as possible in a region with short summers, polyploidy may sometimes be a highly self-adaptive mutation to meet the conditions of a cold climate.

In the second great class of mutations, not the chromosome as a whole but its constituent genes (or units of inheritance) may be changed.

This is the type of mutation that has been most thoroughly worked out in insects, but it seems to be universal. It gives rise to Mendelian inheritance. In this type single genes in the chromosomes are altered and correspondingly single somatic characters or single groups of them are changed. Such changes, once arisen, go down through the generations. In the domesticated plants and animals the numerous races are usually due to this so-called point mutation. But also wild species, as for example the native violets, seem often to depend on this sort of mutation.

The number and kinds of point mutations that can occur in a species is determined by the total number of genes, and this is limited. Consequently the repertoire of variation in any species is limited. Accordingly we find parallel variation in the species of a systematic group. Thus, among mammals, we have albinos in very many domestic and feral species. Hairlessness repeatedly occurs but is best known, naturally, in domestic animals, like horses, cows, guinea-pigs, mice, dogs. Polydactylism is found in dogs, cats, guinea-pigs, mice and rats. Long hair, rough coat, short head, taillessness, varied eye color, are found in many species.

Now an important outcome of experimental genetics is this. Not only do point mutations occur in connection with the maturation of the gametes, but also during the development of the soma. Such a mutation is commonly more or less sporadic in occurrence. One sees it, for example, in a dwarf Portulacca which sends out a branch that is of full size. One sees this somatic mutation in

a red-stemmed plant of which one branch is green. That the germ-plasm has mutated in these branches is clear from the fact that the germ-cells that mature in the full-sized branch of the dwarf, or the mutated green branch, carry genes for full size or green stem, respectively, unlike the gametes of the unmutated branches. Thus the somatic mutations may establish different (but usually not wholly novel) genetic lines.

Somatic mutation is in some cases not sporadic, but its occurrence is regular and controlled. Such mutations are found in variegated plants. Thus one sometimes finds an ear of yellow corn where the kernels over a considerable area are red. The affected area has undergone a somatic mutation from yellow to red.

The process of somatic mutation is well illustrated in the larkspur, Delphinium (as shown by Demereč), where the light pink petals are regularly variegated by darker spots—areas of denser anthocyan pigment. In some strains these spots are small; in such, somatic mutations occur late in the development of the petal. In other strains the spots are larger; the mutation occurs early in petal development and affects a larger area. The point is that a hereditary, racial character is due to an orderly somatic mutation. This case is of inestimable importance. It is proof that differentiation in this case, at least, is due to somatic mutation. But the case is by no means unique, and many patterns in plants are doubtless due to such somatic mutation.

It is alluring to speculate upon this discovery and to formulate the hypothesis that the spotted pattern of mammals, *e.g.*, of the coach dog, and of birds, and the stripes of reptiles and mammals are due to such somatic mutations. Perhaps some day the entire process of differentiation may be found to be due to a regularly recurring series of somatic mutations.

However this may be, in some species, as *e.g.*, Delphinium, the course of embryological development is, in part at least, controlled by precisely the same mutative changes as the course of phylogenetic or evolutionary development. Thus we see that the course of evolutionary development and the course of individual development may be due to the same causes. Where embryological development and evolution are so intimately related we can better understand the significance of the adage referred to above: "Ontogeny recapitulates phylogeny."

What is the cause of this mutation, so fateful for evolution? The late Danish geneticist, Johannsen, based his theory of the pure line upon the idea that sexual mixings are necessary to the production of genotypic differences. According to this theory a homozygotic line that is reproduced without sexual reproduction, like the potato, or by self-fertilization, like the bean, undergoes no mutation. This view is, we now know, chiefly through the remarkable experiments of Banta, too narrow. Banta has carried certain parthenogenetic lines of Cladocera (water-fleas) through 850 generations, reproducing only parthenogenetically. During this time great numbers of mutations have occurred, both of a morphological and of a physiological sort. Thus he has gained many strains quite distinct from that one from which all started. One of the most important results of all those years of experimenting in genetics is the demonstration that the germ-plasm is constantly mutating. These facts are so clear as to lead us to rephrase the old problem of evolution and instead of stating it as the problem of the origin of species think of it as the problem of why any species remains long constant. Perhaps the assumed con-

stancy of species is due to the fact that we have known them for so short a time and that we have really followed the "blood lines" of very few wild species of plants and animals. Certainly the mammalian paleontologist will not find the view of constant rapid mutation unacceptable.

What are the causes of point mutations? How are they brought about? In many cases, as in Banta's Cladocerans and many lines of Drosophila, we seem forced to conclude that mutations may occur spontaneously, that is, from internal causes like the breaking down of the uranium, radium, lead series. From this point of view the cause of evolution is to be found in the nature or constitution of the genes themselves.

However, the changes of the germplasm may be hastened, we have learned in the past few years, by the direct action upon them of X-rays and radium. This discovery of Mavor, Gager, and especially Muller, provides a routine process in inducing gametic mutation now used in many genetical laboratories. And Patterson has recently shown that somatic mutation may be similarly induced.

Not only radiations but also changes of temperature cause a change in the rate of gene mutation in Drosophila, as Muller announced before the American Association for the Advancement of Science three years ago. "Gene mutation," he said, "can be affected by temperature, even as chemical reactions are affected, and the results can be measured."

Recently we have seen an attempt made by Babcock and others to test the hypothesis that it is the radiations of the earth that are inducing mutations in the genes. Drosophilas reared in a tunnel, deep in the mountains where radiation was strong, produced more mutations than in the laboratory, where radiations were feeble. These results seem to be confirmed by the recent experiments of Hanson. They throw light on the rate of production of mutations; they confirm what we know from other experiments, namely, that radiations may accelerate the mutative processes.

Since external conditions, such as temperature and radiations, can control the rate of mutation, the hypothesis would seem tenable that all mutations are so caused. It does not appear, however, that increased temperature calls forth or produces mutations that would otherwise never have occurred, but merely accelerates the realization of potencies wrapped up in the genes. Any chemical reaction is, within limits, accelerated by raising the temperature. The increased temperature is, however, not the cause of the specific reaction, but rather the cause lies in the nature of the interacting reagents.

Also, it seems probable that radiations are not the primary cause of gene mutations but that they accelerate processes that are initiated by the internal structure of the genes.

That the essential nature of gene mutation is determined primarily by internal conditions is well shown by the experiments of Demereč. Thus in Drosophila there is a sex-linked gene that is chiefly responsible for reddish body color. This reddish mutates back to the wild type; but it does so only in females and such females as are heterozygous and it mutates only at the time of maturation divisions. In another mutant of Drosophila (miniature wings) Demereč has found a gene that mutates both in males and females and at all stages of development, so that somatic mosaics are produced. Such behavior of the mutating genes is certainly incompatible with the hypothesis that cosmic influences are necessary to set off the gene mutation. The gene has internal capacity for mutation just as an alarm clock has an internal mechanism for

ringing a bell at a particular time. Gene mutation can arise from the very mechanism of the gene.

Mutation is, however, not the only evolutionary process. Were it the only process, the organic world would now be in a state of chaotic disorder. Hardly two individuals would be even approximately alike and vast numbers would show all gradations from the effective to the most helpless cripples and half-developed monstrosities. In such an organic world the idea of applying to organisms the concept of species would hardly have occurred to a Ray, or of listing and naming species by an improved system to a Linnaeus.

Actually we find in nature a limited number of categories that we call species. Actually the individuals composing these species are not only effective but they are things that are interesting or beautiful in form and color. For the most part these species are more or less sharply marked off from each other.

What is the additional factor which is responsible for this orderly organic world of species?

This factor is one which has brought about a wide-spread adaptation of organisms to their environment, and the mechanism that has brought about this adaptation is the environment itself. To survive, a mutation must meet the conditions imposed by a very puritanical environment. And, indeed, environment is not only puritanical; it is also hard-boiled. If the reward for those who meet the conditions of environment is bliss the penalty of those who fail is death.

Into the rigor of environmental selection the last few years of genetical research have given us a new insight. First of all, it is quite clear that only a part of the gametes are capable of forming zygotes. To commit a Hibernianism we may say vast numbers of individuals perish before they are conceived. Of the zygotes started many are incapable of passing through more than a brief development. Thus in mice half or more of the eggs that are ovulated fail to develop into young mice, and of the young mice born only a fraction mature. Of the potential seeds in the pod or capsule only a part develop, and of the developed seeds only a part germinate when planted under good conditions. In humans the children that are born are a residuum merely of the zygotes that started, and early infancy is the great period of further elimination of the non-viable. Those who live to be parents are a highly selected segregate of the population of zygotes that began development, the grandparents are a still more highly selected segregate. Many are the zygotes that are called into existence, but few are chosen to carry on the race. We esteem the patriarch not because he is old but because environment has so often given him the O. K. Honor thy mother and thy father. Emulate them in being in tune with environment so that like them you may live long in the land.

Genetics is causing a *volte face* in our view of the effect of social work. The physician and the sociologist have been inclined as they look at the world of human unlikeness in body, mind and disease-resistance to find a firm point of departure on a tacitly assumed homogeneous early stage. Differences in behavior of children are ascribed to differences of experience in infancy; and, if you insist that infants are already unlike at birth, then varied intrauterine experiences must be responsible. On this hypothesis the population of early zygotes is uniform; dissimilar environments cause variation. If environment is made uniform then all the longed-for machine-made-uniformity of each product will be achieved. For on this basal assumption the sociologist feels he can rest, and from it he can start: all fertil-

ized eggs of the species are exactly alike.

Genetical experience points just the other way. The foundations of the sociologist are rotten. The population of fertilized eggs is the most diverse of all populations. Life is a process of elimination of the extreme variants—before birth, in infancy, in childhood and youth. The end result—the mature man and woman—is the least variable population of all—certainly in its genetic aspects. Out of great heterogeneity of zygotes has come an approximation to homogeneity in the adults. Environment, acting as censor, tends to keep the species homogeneous and pure.

To recapitulate, the study of genetics has thrown light on evolution first of all by its intensive study of mutations—which has resulted in locating mutation in changes in the chromosomes—the germ-plasm. The study of genetics reveals the germ-plasm as undergoing changes in its composition at particular points and finds that these changes may be hastened among other agencies by radiant energy. But the radiant energy is not the sufficient or a necessary agent. Not sufficient, for only such mutations occur as the nature of the genes permits. Not necessary, since mutations occur that certainly seem to be independent of any environmental change.

The study of point mutation has revealed the fact that it occurs not merely in the process of forming germ-cells but also during the course of somatic development, thus determining differentiation. Indeed, somatic mutation may occur in so regular and orderly a fashion as to be responsible for a developmental pattern. Mutation may hold the key to developmental differentiation.

If gene mutation falls short of explaining all the elements most commonly found in species-formation, all such elements are provided in chromosome behavior. For new chromosomes introduced into the chromosome-complex may set up housekeeping with the old, will bring in many differential characters at one time and will breed true. The new combination will show low fertility with the old or with other new combinations. The characteristic traits of species differentiation all follow the formation of new chromosomal aggregates.

Finally, mutations, though limited in their variety by the nature of the mutating genes, still dart out in the most varied and often little-adapted directions, like the movements of the "it" in "blindman's buff." But progress is made in the direction of adaptation when environment's signals become effective. Genetics alone is incompetent to give a complete picture of the processes of organic evolution. Genetics and ecology, working together, are competent to explain evolution.

# THE BISON AS A FACTOR IN ANCIENT AMERICAN CULTURE HISTORY

By Dr. WALTER HOUGH

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ZOOTECHNY, a term introduced by O. T. Mason to cover the correlation of man with animals, is more comprehensive than the animal industries of G. Brown Goode's classification. The subject is ramifying and has important bearing on man's progress in culture.

It is obvious that in preagricultural times tribes favorably situated as to meat supply enjoyed advantages over other men. Notably, also, grades of culture conformed to the character of the food supply, even those grades having access to animal products being low compared with races depending on the yield of cultivated plants. It is seen that of the groups entering America those connecting with herds of herbivorous mammals, the only animals capable of furnishing an abundant and reliable supply of meat, skins and other useful products, would have a great advantage.

In the high north the reindeer answered the condition in part, the remainder being supplied by fish and aquatic mammals. The migratory habit of the reindeer was a drawback to the tribes of the north whose dependence upon aquatic life required fixed habitations along the shores. For this reason the actively shifting reindeer could not be kept in contact by tribal movements, and a state of dependence on a major mammal was not realized. Incidentally this more comprehensive subsistence induced a more varied or even higher culture than in tribes which became commensals to migrating mammals. The shore-living peoples considered above are typified by the Eskimo.

Migrants from Asia, following the reindeer and reaching the interior, assumed other phases of subsistence midway between that of the Eskimo and that of tribes pushing further south, namely, a rationing of fish and the meat of mammals. We may consider this phase to obtain up to the Slave Lake region where contact with the buffalo could begin.

What would appear to have been the most important event in the life of Asiatic tribes assimilating themselves to American environments was getting in touch with the buffalo. We can say that aside from food and materials needed in the majority of arts, the buffalo was instrumental in diffusing the Asiatic widely and rapidly in America.

Under no other conditions of "infiltration" would these aliens arrive relatively soon at the frontiers of an environment capable of molding civilization that would eventually react strongly on other populated environments in the spread of its culture.

That the buffalo was the important factor in first sustaining the tribes drawn into its range is maintained here. If this theory is accepted it may be advanced, first, that tribes of an early period hunting buffalo would be introduced into the wide stretches of plains otherwise uninhabitable as providing none of the prime requisites of life. Even with the buffalo as sustenance the Plains would be a repressive environment unlikely to produce or maintain anything but a low stage of hunting culture. Second, in this buffalo complex we would have tribes of low culture closely associated with this animal and gradually extending to the extreme limits of the range, say the approximate present

Mexican line. So far as is known now this condition existed for a long time. No tribes of the character suggested appear to have survived to the period of the repopulation of the Plains by northern Indians, which occurred with increased rapidity in the historical times after the introduction of the horse.

Third, we may suppose that in the movements of former tribes with the buffalo some units became peripheral and located in environments capable of human sustenance. It may be inferred that in some instances tribal segregations of this kind would initiate steps leading to higher culture. Of them can be cited the preagricultural Basket Makers, occupying a desert environment, whose arts indicate the genius which became the remarkable Pueblo culture. The period of the agricultural Basket Makers was about 2000 B. C., according to Kidder, whose estimate is based on the early type of maize in possession of these people.

Modern Pueblo research indicates that the people in the grade of the preagricultural Basket Makers had spread widely in America. Already by the efforts of Kidder and by others under his inspiration, the bounds of this rude substratum have been greatly enlarged. The indications are that the range of the Basket Makers will prove astonishingly great.

The presence of Pueblo remains on the Canadian in the Amarillo region, west Texas, furnishes a glimpse of possibilities of the transition of Plains culture into a peripheral culture that penetrated the Pueblo region of the southwest as the initial Basket Maker phase. This theory seems plausible, but its substantiation depends upon an intensive exploration of some of the typical Canadian River pueblos. Some good work has been accomplished by Dr. W. C. Holden, but in a limited degree. Superficially the pueblos seen by the writer under the guidance of Mr. Floyd Studer in the breaks of the Canadian twenty miles north of Amarillo, Texas, resemble those of Pueblo I sites worked by Dr. Frank H. H. Roberts in 1928 in western Colorado. Especially suggestive are the round and rounded rectangular plans defined by stone slabs set on edge. So far no coursed masonry has been seen on the Amarillo sites. Trial excavations by Mr. Studer in house débris deposits show layers defined by charred basketry, small stakes, ashes and mussel shells. Flints, arrow-heads, scrapers and chips are abundant. There is a little pottery of a rude character. Dr. Holden secured a pottery vessel of coarse paste with combed or scratched surface. This vessel is globular with constriction above giving a scarcely defined neck. The vessel has a wide opening. In some respects it is like specimens secured by Dr. Roberts from Chaco Canyon, New Mexico. On the flat land and slopes down into the breaks are innumerable plans in stones, round like tent sites, large circles and rectangular stone-bounded areas. Quantities of variegated flint derived from a neighboring quarry cover large areas and chips dot the surface.

The surrounding scenery reminds one of the Pueblo region with buttes, bare slopes, and sage, yucca, cactus and other desert plant species.

Work is needed here urgently to decide whether this is a Pueblo beginning or, as was thought by former investigators, a backwash from the Pueblos. It is clear in any case that we have here a mingling of Pueblo and Plains culture and a most interesting problem of American archeology awaiting solution.

A more important example is the archaic horizon of Spinden and Gamio in Mexico. The archaic peoples of Mexico made pottery and cultivated maize and, therefore, do not represent the earliest rude tribes of the region who no doubt utilized as a means of subsistence

the agave culture plant primarily and the other useful plants and animals of the diverse regional environment of Mexico.

Before the peopling of Mexico and the relatively rapid growth of arts there it is inferred from cultural remains to the north that the tribes generally were on a low plane belonging to tribal subsistence by hunting. Gradually maize was introduced among them, originating in Mexico perhaps around 3500 B. C., and passed from tribe to tribe over a long period. With reference to the antiquity of maize it may be said that the earliest Maya date, 622 B. C., found that people with cultivated corn. Beyond this to the primary cultivation of maize is conjecture, and a tentative date based on Kidder's estimate of 2000 B. C. for the Basket Makers would be 3500 B. C.

With the tribes in immemorial contact with the buffalo a state of divisional subsistence occupation was superimposed, changing the hunting tribes in less degree than those planted in favorable locations as noted.

The latter responded to a greater degree to the stimulating effect of maize, in fact, the native cultures were profoundly enhanced by this cereal. It may be affirmed that the higher cultures of America began with maize. Also, as the writer has stated in another place, the diffusion of maize was not uniform and at the beginning of the historic period the plant was not yet in the possession of all the tribes, especially those isolated by mountain barriers and having an adequate environmental subsistence of their own.

Returning to the roving buffalo-hunting tribes it may be said that the time here covers a long period characterized by little cultural change. The time is probably long enough to include the ultimate disappearance of all species of bison except the form prevalent in the historic period. In this view it appears that some of the older species now called fossil were on the point of disappearance at the coming of man to America. The cause may have been the crowding out of the older bison by the more prolific modern species. The Folsom find suggests a *battue* in which the dwindling remnants of an ancient herd were slain by ancient hunters perhaps with the throwing stick type of hurling weapons.

The researches of paleontologists have shown that a number of species of bison inhabited America after Pleistocene times. Necessarily the ranges of the older species are not known, as all information is yet fragmentary, but it is likely that the species distribution became local in its later phase and much more restricted than that of the historic buffalo. This idea is based on the grassing of sedimentary deposits following the last glacial epoch remaining in place long enough to produce a soil cover suitable for the growth of grass species. The correlation of buffalo species with grass species also is suggested as a well-known phenomenon of natural life relating to such dependencies.

There is suggested here the probability of cropping ranges at different geological periods on the Plains and low interior lands. So far as is known, the development of the ancient species of buffalo took place on the same character of terrain as that forming the range of the historic bison and would be coincident with the extensive grassing of alluvial deposits. It is probable that the conditions were best for the vast increase of the latter species, but an authoritative statement can not be made.

It is most likely that the contacts of Asiatics in America were with species of buffalo antedating the present form continuing with the current species. The conditions in general appear to have forced the old species of bison, coming over the land bridge from Asia, southward to extinction and late survivals in

favorable areas, as appears to be shown in Dr. Gidley's exploration in Florida. Other conditions led to the growth of the present almost extinct species into the form described by Cabeza de Vaca in the fifteenth century and seen in such prodigious numbers by the first travelers to the interior of America.

Some years ago it was not perceived that the contact of Asiatic settlers in America would have been with the extinct species of bison. But recent discoveries put a new light on their early contacts in America.

Leaving the theoretical prelude, suggestions and possibilities aside for what they are worth, some tangible facts may be taken up. Several years ago at Folsom, New Mexico, artifacts consisting of chipped stone points were found with the remains of a number of specimens of an extinct buffalo. Owing to the many unproved, doubtful and scientifically unchaperoned "finds" that had been announced disappointingly through a long term of years, this discovery was viewed with proper caution.

In this case, however, the find was in the hands of scientific men and the work was carried on in a thorough manner. There is no room for doubt that the stone points of some weapon were observed in position among the skeletons of the extinct buffalo. This fact is indisputable. Beyond this the conclusions that have been advanced as to the age, the position in the geological scale of the deposits in which the artifacts were found and their particular type are as yet tentative. Dr. Adolf Penck, the weight of whose authority on valley cutting and fluvial terrace deposits is internationally known, would put the age at not more than 9500 years, and this figure he regarded as tentative.

The Folsom discovery is placed to the credit of Harold J. Cook and J. A. F. Figgins. Dr. Barnum Brown, of the American Museum of Natural History, conducted the subsequent work for that institution. Since the connection of man with the buffalo at Folsom is based on chipped stone projectile points of a certain class, some remarks on their manufacture and type may be introduced here.

The form, which is a thin blade spoon-shape at the forward end and worked out into an arc at the base, is widespread in America and is distributed over the world sparingly as to the National Museum collections of Old World archeology.

The flint worker chipped the blade into shape normally and the unfinished implement had a straight base. The base was then worked into the arc of a circle by flaking from either side alternately, as in making the nocks on an arrow-head. In order to thin the bowed median axis of the implement a broad flake was taken out on both sides, producing a reasonably flat surface for hafting. Almost all stone arrow-heads worked in from the base show flakes taken off vertically. Flint flakes, or blanks, can not be struck off with biconcave surfaces, although one surface shows a concavity. By the method described the flat side of the flake can be hollowed.

It is suggested, therefore, that the biconcave Folsom blades were made of this type on account of the thin hafting desired, as the like blades of Ohio, Georgia and other localities in the United States. It would seem that there is little racially, tribally or geographically characteristic connected with the Folsom blades.

At Frederick, Oklahoma, a similar discovery of the association of man with an extinct species of buffalo was made by Mr. A. H. Holloman, owner of the sand pit, and examined by C. H. Gould, state geologist. Owing to conditions which made the find less clear than that

of Folsom, the discovery was at first discredited, but subsequently regarded as authentic, though not very ancient, especially as it is in line with the former work and not one of the many tantalizing isolated finds.

It would seem that the discoveries of traces of man in beds assigned to Pleistocene have put scientific men on the horns of a dilemma. The crux is that man is very ancient in America or the beds recent, that the animals found carry man back very far or that the cultural objects declare for recency of man. Impartially viewing the evidence, it seems that the beds are recent relatively and that archeology is more concerned than geology in the findings.

Thus the archeologist applying the culture development schedule of his science would believe that the finding of arrow-heads, pottery and grinding stones in undisturbed fossil beds as at Frederick, Oklahoma, would indicate that the beds date from a comparatively late period of human arts.

In fact, the material so far taken out with the remains of bison and mammoth would fit in best at the Neolithic period. The Neolithic besides is a master period in which there was world-wide distribution of man over the earth. American tribes as they came into the light of history were Neolithic in arts and physically typical Indians.

Nevertheless latitude must be given to the supposition that the end term of migrations before the European Neolithic, progressing slowly on the long march toward that phase of art, may have reached America. Also it is not supposable that all increments arriving at different times in America possessed the same grade of Neolithic culture or the same physical characteristics.

The unity of the Indian race inhabiting the New World at the beginning of European contacts is now taken for granted. This view follows many years of discussion of various traits dealing with dissimilarities and resemblances which were supposed to yield classificatory data leading to the naming of many races within the stock. With the advent of physical anthropology was established the fact that from one pole to the other the Indian varied only slightly within the limits of a physical unit type.

The first implication from this unity is the ruling that the wide distribution geographically of a uniform type is proof of comparative novelty of its diffusion. It is also assumed that considering the historic Indian the initial migrants coming into America over Bering Strait were of that stock whose increase populated the western hemisphere. While this is the observed result shown in the Indian, the original uniformity is conjectural. There is a possibility that diverse stocks may have come in at different times and have finally merged into the known Indian or passed out without trace as yet.

Countering this is the fact that all skeletal remains so far found under conditions denoting presumed antiquity are shown by Dr. Hrdlička to conform to the Indian physical type. This is in obvious contrast to results in Europe and may be considered safe ground for the conclusion that man in America has no ancient or geological history of moment.

# THE CULTURE OF THE BAYA TRIBE OF WEST AFRICA

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THE Baya tribe is located in French Equatorial Africa in the upper Congo basin southwest of Lake Tchad. This tribe is composed of about 200,000 natives. According to Froebenius and Ankermann, they belong to the West African culture area, which comprises all the Bantu peoples of the Congo River basin and its tributaries. Bantu tribes living in close proximity to the Baya are the Karre, the Mandjia, the Kaba, the Dapa and the Banda. Each of these various tribes has a population ranging from 50,000 to 200,000.

### I. Climate, Geography, etc.

This section of the country covers an area of about 150 square miles. As a whole the country is rolling, although in places there are mountains. The elevation averages about 1,600 feet above sea-level. There are some large rivers, especially the Ouham and Sanga, but they are not navigable for very long distances because of the many rapids. The soil is very rich owing to its virginity. Tall grass, some of it twelve feet high, covers the whole area. Dwarf trees are abundant. Every year the natives burn the grass and this stunts the trees. Along the streams there are dense forests and undergrowth. There are two seasons: the rainy season, which occurs from April to November, and the dry season corresponding to our winter. Both seasons are hot. The sun is dangerous because of the direct or actinic rays, although the thermometer does not register much above 90 degrees in the shade. It is necessary for all northern Europeans and Americans to wear helmets of cork or pith.

There are many swamps, and consequently malaria is ever present, especially with the white people. Along the streams and other low-lying country is found the tsetse fly, the great scourge of Africa. This fly causes sleeping sickness, for which science has not yet found an altogether satisfactory remedy. At times whole herds of animals die because of becoming infected with this disease.

### II. Manner of Living

Formerly all villages were located on hilltops and were protected by hedges of cactus. Recently the French government has compelled all villagers to move to more accessible regions. The natives live almost exclusively in villages. Isolated dwellings are rare. The country is sparsely populated and between villages there are miles of uninhabited regions. The village stands as a unit. Each individual thinks of himself as related to the others. Often there is war between the villages because of the stealing of wives or property.

Houses are cone shaped and usually about nine feet in diameter. The walls are made of woven mats, or at times of clay. The roof is made of thatch and the floor of clay beaten very hard. There are no windows and only a small opening for the door. The inside is partitioned off into two or three parts; one part to sleep in, a second in which to store pots, etc., and a third for the fireplace.

The granaries usually consist of large baskets about four or five feet in diameter and elevated on wooden posts to keep out insects and small animals which are ever present. Chicken houses are also elevated in the same way to keep out wild animals.

The men's clothing consists of loin-

A GROUP PICTURE

NATIVE GIRL GRINDING FLOUR

NATIVES MAKING A MAT

cloths woven on crude little weaving machines, which are made by driving several sticks in the ground. They raise cotton from which they spin their thread. Thread is spun with a very crude spindle made by taking a stick and weighting it at the lower end with a wheel made from a piece of baked or burned clay. Sometimes they wear loin-cloths of bark which has been peeled from certain selected trees. They soak this bark for several days and then it is beaten until it becomes thin and soft.

The women usually wear large bunches of leaves, but there are some who also wear a small apron made of cotton threads. Near the trading posts they are beginning to buy cheap calico whenever they can afford it. The native is very proud of any clothes he can obtain even at the expense of making himself look ridiculous. Once a man was seen going down the road walking very proudly because he was lucky enough to possess a fan and a dilapidated umbrella.

The men usually cut their hair short but at times they braid it. The women also generally bob their hair, but at times they braid it in many tiny small braids close to the head. In times of bereavement the head is shaved.

Beads are worn in profusion, the brighter and the more colors the better. The women wear them around their waists, around their necks and on their arms. Bracelets, anklets and crude rings are worn. The nostrils are pierced and on both sides long ornaments are suspended. A hole is pierced in the lower lip and an ornament placed through it.

Tatooing is done extensively throughout the tribe but seemingly without signification.

### III. Occupation

Agriculture is carried on extensively by the women. The two staple foods are cassava or manioc and kaffir corn, which are beaten into flour and mixed in hot water until it becomes like dough. After it is baked it takes the place of our bread. Yams, millet, pumpkins, beans and cucumbers are grown extensively. Small patches here and there are cultivated by hoes with short handles. They do not use fertilizer but clear off a new garden spot whenever the old one wears out.

Both spinning and weaving are done

by men. From cotton they spin the thread for their loin-cloths. They also weave baskets and mats.

Mats are woven from strips cut from palm-leaves and certain kinds of grass. They are used for beds and floor mats. Some of them are beautifully made, having designs of different colored strips.

The natives make many different kinds of baskets. They employ the same methods as they do in making mats, although in some places coiled baskets are made of reeds. They also make hats and bags which necessitate slightly different methods from those used in making mats.

Pottery is developed quite highly in this section of the country. The making of pots is always the work of the women. They make pots of all sizes ranging from those large enough to hold a pint to those that hold about ten gallons. Most of them are decorated with crude designs. The pots are made of a special clay and are burned in a charcoal fire after shaping.

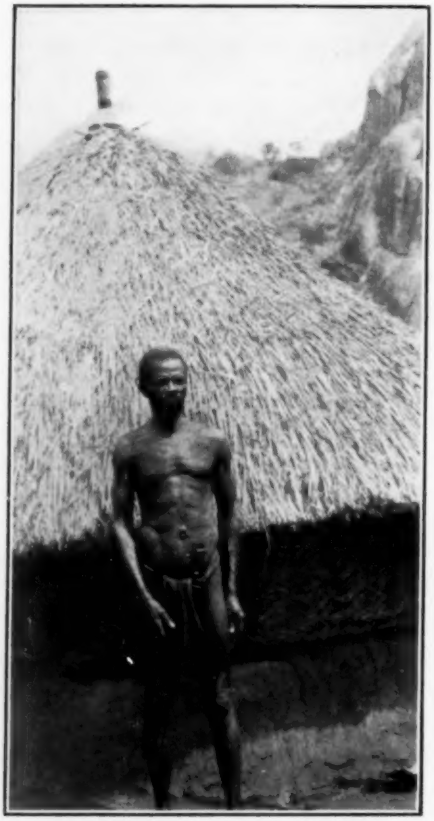
Chickens are domesticated but are much smaller than in America. Goats are kept, not for milk, but for meat. They are also used as a medium of exchange in buying wives. They have some small dogs which are very skinny, as the natives take away any small animals which they catch. There are a few horses scattered throughout the country. There are no cattle because of the tsetse fly.

The Baya are great hunters. The grass is burned by the natives every year and many animals are speared as they try to escape. They throw their spears very accurately and rarely miss their animal. They also use the bow and arrow and at times poison their arrows, rendering them very deadly. The traps are made by having a noose on the end of a rope. A hole is dug in the trail of an animal and the noose placed around the circumference of the hole. It is concealed with sticks and leaves and the free end of the rope is tied to a sapling. The animal walks into it unaware. Sometimes they dig a deep hole in the ground, concealing it by placing sticks and leaves over the opening. The animal falls into it and can not get out. In this region one finds many different kinds of animals, such as elephants, hippopotamuses, crocodiles, lions, leopards, buffaloes and antelopes.

Fishing is an important occupation among many of these people, especially those who live along the large rivers. Sometimes the native climbs up in a tree and crawls out on a limb which reaches over the water and waits for the fish to swim by. When a fish comes within range he throws the spear at it. Oftentimes there is a hard battle to land these fish because of their extreme size.

Canoes are made by digging out the inside of a palm log or other logs. Paddles are used for rowing. In some places the natives stand up and row in unison, often chanting rhythmically as they row.

Metal working is a very important occupation. The natives mine their ore, smelt it and then pound it into tools or money. The blacksmiths are the gifted men of the country. There are only a few of them and they are looked up to and respected, for "No blacksmith, no spears; no spears, no war." Knives are at times decorated very beautifully and require some skill in making. The people pride themselves on their spears and take a great deal of time in their manufacture. They also make arrows, bracelets, hoes, axes, throwing knives, hole diggers, adzes and other small articles. Their money is sticks of iron about twelve inches long by one sixteenth of an inch thick by about a quarter of an inch wide.



A TYPICAL BAYA MAN

### IV. Social Organization

The population of the villages ranges from fifty to two thousand people, but the average village has only about three hundred natives.

The country is ruled over by chiefs, each chief having a village under his control. The chief has the right to compel his people to work for him and supply him with food. If they refuse he either flogs them or has them jailed. Each subject helps to support the chief by bringing him a part of his produce when harvested. The chief receives his share of every animal killed. Each chief, however, is responsible to the French government and must see that all his villagers pay their taxes and do a certain amount of work for the government. The chieftainship is inherited by the eldest son on the paternal side.

Sometimes a village is composed of several sections, and as the chief can be at only one place at a time he sets captains over these different divisions. They rule as chiefs in their own division but are responsible to the head chief of the village.

The tataways are assistants to the chief who help in the enforcement of law. Usually they are the friends of the chief or the influential men of the village.

In Baya land whatever an old man says is respected and usually obeyed. It is the old men who cling to the old customs and traditions. They are the teachers of the young. Sometimes an old woman is looked up to as the "mother of the village."

Marriage is exogamous, the members of a village usually marrying outside their group. A man seeks a wife in a neighboring village. The marriage is arranged by the fathers or elder brothers of the young people. The girls are bargained for when they are not more than a year old. The bride is usually taken when she is seven or eight years old. The purchase of a wife is quite an undertaking and oftentimes expensive. A wife may cost as much as four hundred sticks of native iron money, two goats, four spears and two knives. If the man seems anxious for the girl, the mother-in-law may intervene and demand that the young suitor come and carry wood and water for her for a week, or perhaps the mother-in-law may want the mother of the groom to make her some salt. If the wife dies without having any children, her father must forfeit the bride price or give another daughter in her place.

Polygamy is common. A man's wealth depends upon the number of wives he possesses. Chiefs have been known to have as many as thirty-six

wives. Many men have two or three wives, but ordinarily they can afford only one wife. A man is loath to give up his many wives, for as he says, "It is all right when my wife is well, but should she get sick, what would I do for food and who would make my garden, if I only had one?" Each wife has a separate hut where she lives with her children. The wives are glad for their husband to have several wives because it lessens the amount of work each has to do.

Divorce is at the will of the husband or wife. If the wife leaves her husband he can demand the return of the bride price.

Private property in land is recognized; nevertheless, each village has a particular section of the country which belongs to it for hunting purposes. When a man dies the relatives inherit the property and his wife receives nothing. The son, if there is one, usually becomes the heir.

## V. Religion

Everything and every place is animated with evil spirits. Gacora is the highest one of these spirits. He is the author of evil and is feared by all. They have no conception of a God of love as we have, but one of terror. When some one dies the relatives say it is because of an evil spirit in one of his enemies. The witch doctor determines who the guilty party is and then the relatives of the individual proceed to punish the victim. They may put red pepper in his eyes or force him to drink the poison cup. Flogging is also used as a means of punishment.

There are many sorcerers or witch doctors who are greatly feared by all. If some one is sick the witch doctor determines who the guilty party is. He places certain foods on the taboo list. He is supposed to heal the people of their diseases by manipulating his charms. If a married couple prove childless, they may take a chicken to the witch doctor who sacrifices it. As a result of this ceremony they are blessed and will be able to have children.

The natives believe in blood sacrifices and offer them for many different things. When a member of the family dies, the head of the family takes an offering to Gacora to appease him so that he will not take another member of the family. When an animal is killed, certain parts of it, such as the tenderloin and liver, are sacrificed to the god of hunting in order that the devotee may possess great skill in hunting. Every year they have a "feast of the dead," when they offer up many chickens and goats to the evil spirits so that they will not claim any more of their number by death.

Charms and fetishes are worn in profusion, each one having a different meaning. A string of small bones was removed from a baby's neck when it was brought to the dispensary for treatment. The mother said she had put these on to keep the baby from taking cold and pneumonia. A young man wore a particular kind of carved stick on his wrist while journeying through the jungle. Upon questioning, he said that it was to prevent the lions and leopards from killing him. Charms are also worn to give the wearer strength.

After death, they believe, the body decays. The soul separates from it and wanders around in the jungle seeking for an opportunity to cause trouble to some one. The soul becomes associated with the evil spirits. Their idea of the after life seems very vague and mystical.

## VI. Art

Art is little developed, although there are some realistic designs crudely made on their houses or earthenware pots. Beautifully colored designs are woven in their mats. There is a small amount of ivory carving which has reached a high stage of perfection.

# TERRESTRIAL MAGNETISM FROM THE VIEW-POINT OF THE ENGINEER

By N. H. HECK

CHIEF, DIVISION OF TERRESTRIAL MAGNETISM AND SEISMOLOGY, U. S. COAST AND GEODETIC SURVEY

THERE are going on at the present time several experiments in new forms of government. Whatever may be the outcome of these experiments, one of the strong elements of stability is the fact that the United States, after 150 years, is functioning without radical change. Originating in a world which regarded war as an essential part of national existence and having itself found it necessary to take part in a number of wars, it is now pressing strongly to eliminate war. These things are not the result of chance but are directly due to the fact that the discovery of America came just when it did. Political events of the succeeding period, more than at any other time in European history, developed in such a way that some of the strongest and best elements of the population left to try their fortunes in the new world. This was possible because of Columbus, and, back of him, because of one of the most important discoveries and inventions of history, namely, the discovery of terrestrial magnetism and the invention of the magnetic compass. While it is possible that these came from China, it was at this time that they first became known to Europe.

In recent years a new device, the gyrocompass, has come to the aid of the mariner and has in part replaced the magnetic compass, but the latter will still continue to be used and all mariners' charts carry magnetic information. However, if not another magnetic compass were to be used, the debt of mankind would still be great to the apparatus which first made it possible to cross the oceans.

The early colonists in this country had more land than they knew what to do with, but as time went on it became necessary to survey the land and mark the boundaries, as is evidenced by the fact that George Washington did some of this work. The magnetic needle in a form different from that used at sea but embodying the same principles made the work possible.

Engineers now have much more precise instruments, but we must remember that in those days not only were precise instruments and men who could use them not available, but even if they had been, the value of the land would not have warranted the expense. There are, therefore, thousands of land surveys made by magnetic methods in all parts of the country, and the Coast and Geodetic Survey is called upon daily for information from its store of records which will aid in repeating old surveys or searching for lost corners. Here again we see an important use of knowledge of the earth's magnetism.

In our time we have had other pioneer performances, not perhaps comparable with the voyages of Columbus but still highly significant, in the conquest of the air. Here again terrestrial magnetism has come into play, and the magnetic compass either of the globe type or the earth inductor type have become the best direction-indicating devices which are available everywhere and under all conditions. Lindbergh used a magnetic compass in his historic flight. All the airway maps carry magnetic information as a result of wide-spread demand from air navigators.

Useful accomplishment, a criterion which appeals especially to the engineer, has been shown. Other practical applications will be described, but it is first

necessary to enter a field which at first thought appears to belong exclusively to the mathematician and the physicist. However, engineers are becoming more and more interested in research and will have to become accustomed to entering such fields, or at least learn what is being done in them. I feel that it will be of interest to consider a problem in research which is of a different type from that usually engaged in by engineers.

In the applications that have been mentioned, it has not been necessary to consider the intricacies of terrestrial magnetism. An analogous case is the use of the relatively simple conception of elastic limits and breaking stresses in materials, while investigations indicate that these are only approximations.

There are few more complex subjects in the range of physics than terrestrial magnetism. Years ago it was thought that the great variety of tides in the different parts of the earth could not be reduced to law, but mathematical treatment has completely triumphed. This is not even remotely true in the case of terrestrial magnetism, and though every resource of the rapidly developing knowledge of physics has been utilized, we are still at the threshold of knowledge. We might be nearer solution if we had centuries of accurate observation instead of little more than a quarter century (considering the earth as a whole). By solution I mean not only full explanation of the phenomena but knowledge of the laws so that prediction may be possible.

The failure to respond to the world-wide attack on the problems is not due to lack of organization or hesitation to apply every possible new method. Something has been done in nearly every country and especially in Great Britain, France, Germany, Canada and the United States. In the United States, the magnetic surveys have been made by the Coast and Geodetic Survey, and the records of its five observatories have, in addition to the immediate purposes for which they were taken in connection with the activities that have been described, proved of fundamental importance in all studies of the nature of magnetism and its governing laws. The Department of Research in Terrestrial Magnetism of the Carnegie Institution of Washington (hereafter referred to as the Carnegie Institution and in some places abbreviated C. I. W.) was organized for the primary purpose of studying the earth's magnetism. It found insufficient information on hand for its purpose and has had a remarkable record of collection of information in all parts of the earth where it was not being done otherwise, and especially at sea through the work of the non-magnetic ship *Carnegie* and the brigantine *Galilee*. While thus a vast amount of magnetic data has been collected over the earth's surface—both land and sea—nevertheless, in view of the changes in terrestrial magnetism constantly going on, this organization like all others must continue to make repeat observations at suitably selected stations from time to time and must operate observatories at places not otherwise provided for. However, since the period of intensive collection as a major function in delineating the distribution of the earth's magnetism is now past and observations may now be generally restricted to the needs of secular variation determination, ever-increasing additions to the fine accomplishments in the field of research may be expected.

Coming back to the general statement of the problem, the first need is for accurate data over long periods. Accuracy in this case means all that the term can imply. The unit of measurement of magnetic intensity is the gamma, .00001 of the small c.g.s. unit, the gauss, and in some computations one hundredth of a gamma is used, so that we are dealing with quantities that are comparable respectively with one hundred-millionth and one ten-billionth part of gravity. While the accuracy of one gamma is not always attained, sustained effort is made

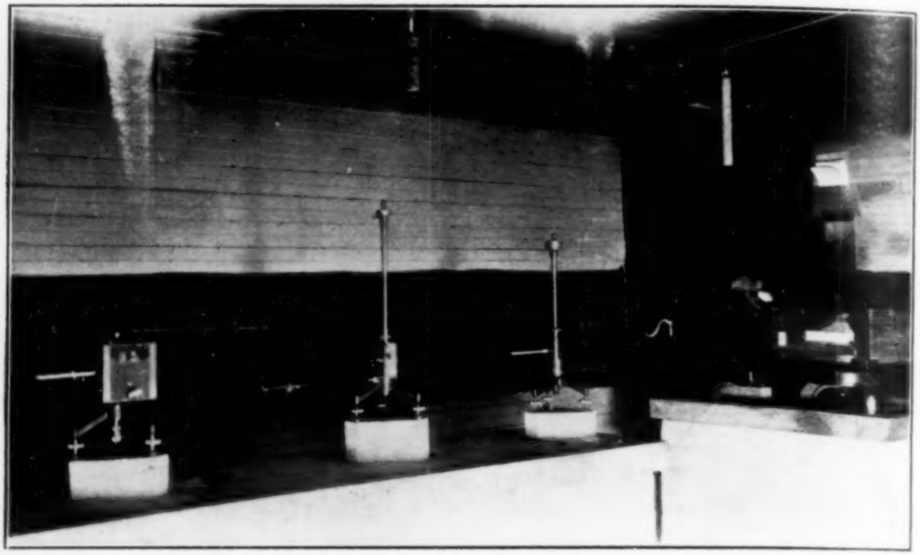


FIG. 1. MAGNETOGRAPH
CONSISTING OF THREE VARIOMETERS AND PHOTOGRAPHIC RECORDER.

to keep the uncertainty of the value as nearly as possible to one gamma. Observations of this order of accuracy are not infrequent in the physics laboratory, but in very few cases are they maintained over a quarter of a century. Time alone brings instrumental troubles, and bad weather conditions have their effect. In spite of these, three observatories of the Coast and Geodetic Survey have unbroken series of accurate observations for more than twenty-five years — those at Cheltenham, Maryland; Honolulu, Hawaii, and Sitka, Alaska.

The methods of observation are described in various publications, and there will be given here only sufficient outline so that the research problems in connection with them may be understood. One set of instruments records continuously by photographic methods the changes in the magnetic elements, declination, horizontal intensity and vertical intensity and, therefore, indirectly the dip and the total intensity. Other instruments are used to determine at regular intervals, as once a week, the absolute values of the declination, dip and horizontal intensity and, therefore, indirectly the vertical and total intensity. We then have absolute values for certain points on the continuous photographic records and, accordingly, it becomes possible to obtain the absolute value at any time desired.

The instruments for recording changes are known as variometers and a complete set with a recording unit as a magnetograph (Fig. 1). Though the variometers vary in appearance and details, they have the common feature that a magnet is suspended in such a manner that it will move from a position of rest only in response to changes in the particular force whose direction and intensity is to be measured. In the case of declination the magnet is free to take the direction of the horizontal component of the earth's field. In the case of horizontal intensity a small magnet suspended by a quartz fiber is held in the magnetic prime vertical by means of torsion in the fiber. A fixed magnet at a suitable distance controls the sensitivity and, as a recent development, another auxiliary magnet (Fig. 2), also fixed in position and having a suitable relation to the

suspended magnet, compensates for temperature. One of the important factors in securing results of the desired accuracy is the determination of the effect of the changes of temperature of the recording magnet and making corresponding correction of the results. By the method referred to, the auxiliary magnet and the suspended magnet are affected the same amount by a given temperature change but in opposite directions, so that the effect of temperature change is eliminated.

The prevention of slipping of the quartz fiber in its cementing medium was an engineering problem on a microscopic scale. While the forces are very small (fiber usually about 0.05 mm in diameter) the holding surface is so small that the forces causing slipping are relatively great. A solution has been found by heating the ends of the fiber and bending them at right angles to the fiber before imbedding them in the cementing medium.

The magnet system used for measurement of vertical intensity (Fig. 3) includes a magnet something of the shape of the walking-beam on a side-wheel steamer, the magnet being mounted on steel pivots which rest on agate planes. Since the magnet would tend to take a position with its axis in the direction of the earth's field, counter-weights are used to keep it horizontal. It might at first thought seem that there would be slipping and change of position of the pivots on the planes, but there is none because all the effective forces are vertical. In view of the perfect balance required, which is more accurate than that of the best analytical balances, it has been found necessary to fasten the counterweight rigidly in position and to use pivots of a special alloy. The load on the pivot points is on the order of twenty tons to the square inch.

Each magnet has a mirror attached and each variometer has a fixed mirror. The same beam of light goes to each pair of mirrors and then back to a clock-driven drum carrying photographic paper. We, therefore, have three pairs of lines, each variometer mirror producing an irregular line and each fixed mirror a straight line which serves as a base line from which to measure ordinates (Fig. 4).

The building which houses these instruments is built so as to keep temperature changes within small limits, and chemicals are used to reduce the humidity if necessary. In extreme cases, as in Porto Rico, a special casing is built around the variometers to make the air space from which the moisture must be removed as small as possible.

We are accustomed in many branches of science and in engineering to new and better instruments and methods, and it is an interesting contrast that the method suggested by Gauss about one hundred years ago is still standard for absolute observation of declination and intensity. Though with the development of standard coils and resistances electrical meth-

FIG. 2. HORIZONTAL INTENSITY VARIOMETER

EQUIPPED WITH APPARATUS FOR TEMPERATURE COMPENSATION.

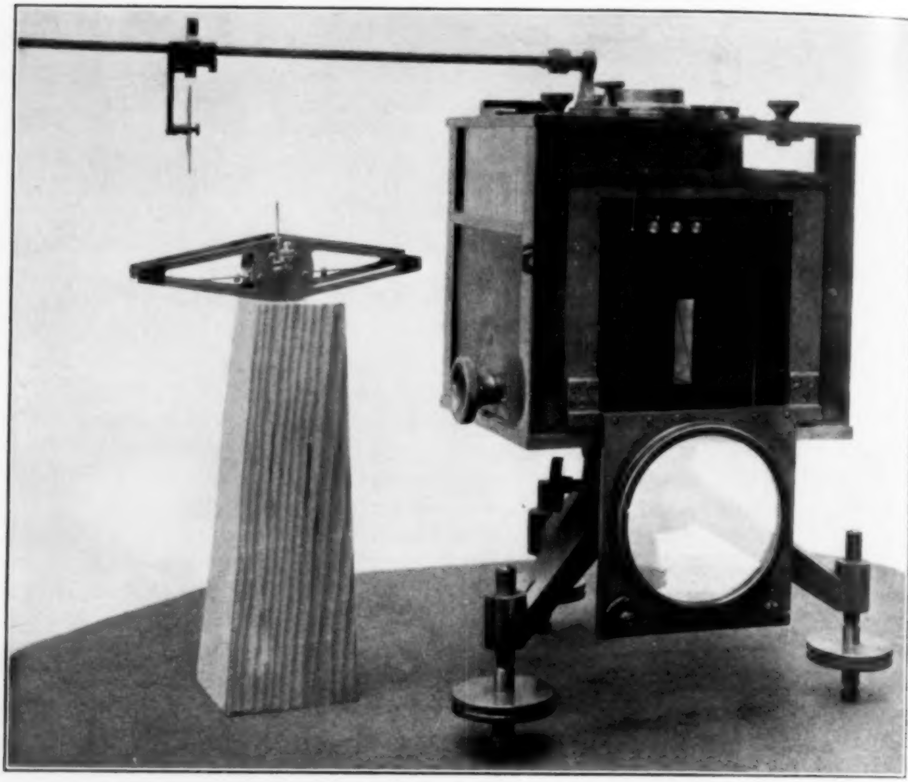


FIG. 3. VERTICAL INTENSITY VARIOMETER
SHOWING RECORDING MAGNET AND OPTICAL SYSTEM.

ods are now available, they have not yet replaced the Gauss method, though the latter has the disadvantage that the observations cover so long a period that the element being measured is likely to change during the measurement. The difficulty in the case of the other methods is that, since the gamma is so small, the electrical measurements must be of an accuracy which it is difficult to obtain at all times.

The absolute measurements of declination and horizontal intensity are made by means of the magnetometer (Fig. 5). Magnetic declination is obtained by determining the angle between the direction taken by the suspended magnet and the direction of a fixed object. The true bearing of this fixed object is determined by astronomical observation, and the difference of the two angles thus obtained is the magnetic declination. Determination of horizontal intensity is a double operation, one part being the precise measurement of the time of oscillation of a magnet suspended with its axis in the direction of the horizontal component of the earth's field and the other part the determination of the position taken by the suspended magnet due to the combined effect of the earth's field and that of a fixed magnet. The first gives the product HM and the latter the quotient H/M, and the two equations thus obtained can be solved for each. (H is the horizontal component of the earth's field and M is the magnetic moment of the suspended magnet.) Recently it has been found possible to measure the time of oscillation without using the eye-and-

ear method. A light beam is reflected (Fig. 6) from the polished end of the suspended magnet through a slit so that for an instant during the swing the beam strikes a photoelectric cell. This is recorded directly on a chronograph and the interval between successive swings can be accurately measured (Fig. 7). The problem was far more difficult than in the case of recording time of oscillation of a clock pendulum, since the oscillation in this case is limited two degrees and since it is necessary to have the apparatus far enough away from the magnetometer to remove magnetic effect at the suspended magnet and yet there must be enough intensity of light to operate the photoelectric cell.

For accurate measurement of the dip the earth inductor is used (Fig. 8). This is in effect a small dynamo coupled with a galvanometer. When the coil is revolved with its axis in the direction of the magnetic lines of force no current is produced, though in every other position there is current. With a very sensitive galvanometer, it is possible to make not only an accurate determination of the dip but also of the magnetic meridian and, therefore, of the declination. The last is the general principle on which the earth inductor compass works.

The transformation of the variation curves into hourly absolute values formerly required four distinct operations in the case of intensity. The ordinates were measured in millimeters, then multiplied by the scale value or number of gammas corresponding to a millimeter of ordinate. This gave the difference in gammas between the curve and the base line, and to this there was still to be applied the absolute intensity in gammas corresponding to the base line and the correction for temperature. The Coast and Geodetic Survey has developed methods whereby but one operation is necessary after certain preliminary settings of the apparatus have been made, and all this work is done at the observatories, except that the necessary settings are controlled from Washington and the results are reviewed there and put in form for publication. Temperature effects are eliminated by a method which has already been described. The conversion of

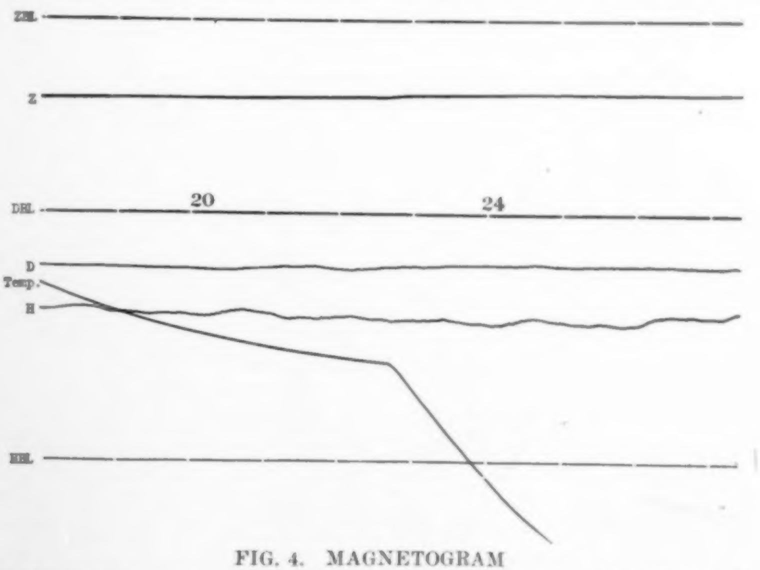



FIG. 4. MAGNETOGRAM

SHOWING TEST OF TEMPERATURE COMPENSATION. TUCSON MAGNETIC OBSERVATORY, NOVEMBER 18, 1929.

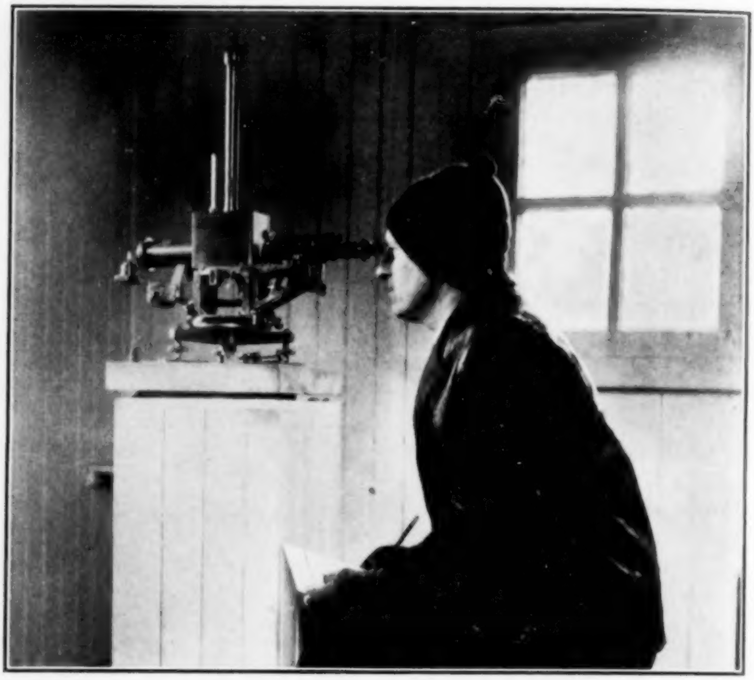


FIG. 5. MAGNETIC OBSERVATIONS
WITH OBSERVATORY MAGNETOMETER AT SITKA, ALASKA.

ordinates to gammas is accomplished by having linear scales for each scale value prepared by photographic means. By suitable arrangements the base line value is applied at the same time that the ordinate is read. At every step labor-saving devices are used to minimize the very great amount of detail necessary, and finally publication is by photographic reproduction of the sheets resulting from the final tabulation so that proof-reading is kept to a minimum and the cost of publication is considerably less than by printing.

The source of accurate information regarding the earth's magnetism and its changes comes from the records of forty to fifty magnetic observatories and also from observations of absolute values at points scattered throughout the earth. While the latter observations are not continuous, "repeat" observations are made at a certain number of them (as, for example, two hundred to three hundred in the United States) at intervals of five to ten years to determine secular change. With the loss of the non-magnetic ship *Carnegie*, whose tragic destruction last year brought to an end observations which had extended over a period of twenty years, the problem of securing repeat observations at sea has become a serious one, and it will at least become necessary to make more intensive repeat observations at land stations near the sea to make up in part for her loss. It is to be hoped that a means may be found for carrying on further observations at sea.

Though there are probably sufficient magnetic observatories to meet all needs if they were well distributed, this is not the case, as a very large proportion of them are in Europe and large areas in other parts of the earth are without observatories. Those operated by the

Coast and Geodetic Survey are well distributed, being in Porto Rico, Maryland, Arizona, Alaska and Hawaii, and those operated by the Carnegie Institution of Washington are in Peru and Australia. There is a definite trend on the part of European countries toward placing observatories in colonies so as to produce a better distribution.

However, even though conditions might be improved in certain ways, there is at hand a mass of accurate material on which to base studies. The first method of attack is the statistical, especially the correlation of magnetism with other phenomena. This is the field of the mathematician and his work is of fundamental importance. The mathematician can accomplish much, but he has to guard himself from the temptation, in treating complex physical phenomena by mathematical analysis, of thinking that he is explaining the physical facts while he may merely be fitting the observed facts to his preconceived ideas. It is not unlikely that this has been done in some cases.

However, it is to mathematical analysis that one of the most outstanding of the many contributions of L. A. Bauer and his colleagues of the Carnegie Institution is due. This is the accepted hypothesis that the greater part of the magnetism is due to causes within the earth and only a small portion to outside causes. Another very interesting and important investigation by the same organization is the correlation between sun-spots and magnetic storms. The results of this investigation (Fig. 9) are especially interesting in that, while the correlation over a period of years is good, individual storms occurred without sun-spots being visible and sun-spots have appeared unaccompanied by magnetic storms. A recent example of the latter case occurred in January, 1930, when with sun-spots of notable size and extent

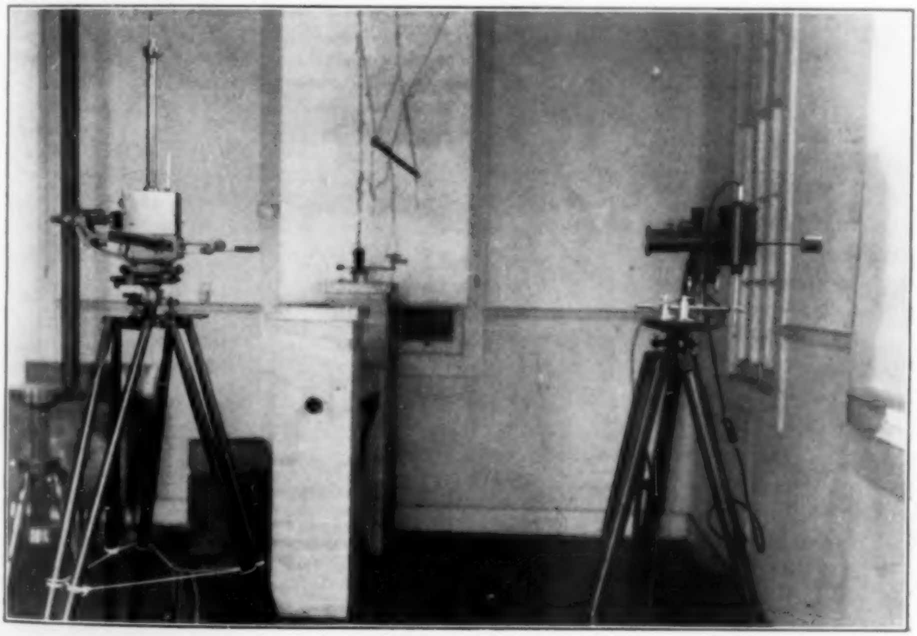


FIG. 6. ARRANGEMENT FOR RECORDING ACCURATELY
THE TIME OF OSCILLATION OF A MAGNET, BY LIGHT SOURCE, MAGNETOMETER AND PHOTO-ELECTRIC CELL.

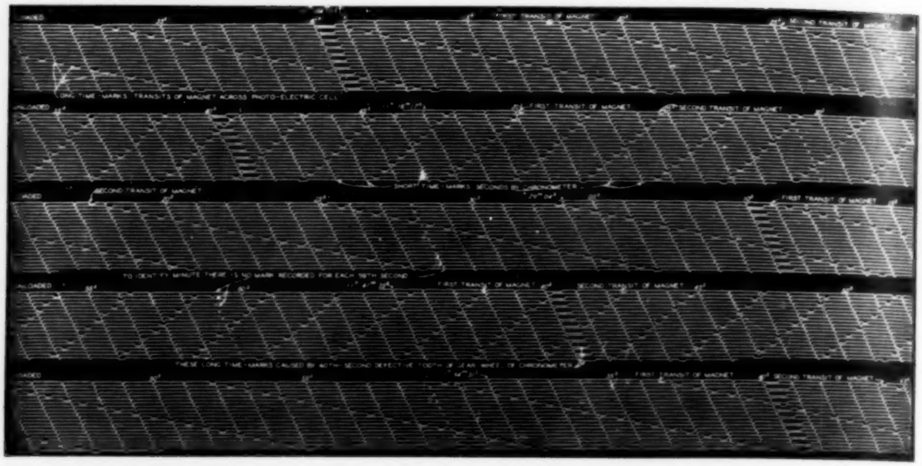


FIG. 7. MAGNETOCHRONOGRAM
OBTAINED AT CHELTENHAM MAGNETIC OBSERVATORY SHOWING RECORDS OF TIMES OF OSCILLATION OF A MAGNET.

there were no magnetic storms of importance. However, all large magnetic storms seem to be associated with sun-spots. It appears that we are dealing with some common cause of both phenomena.

Knowing the close relation between magnetism and electricity, it is an obvious question whether there is any relation between the earth's magnetism and lightning. It is probable that no relation can be traced, but this is not true of the other forms of atmospheric electricity. Here there are correlations over long periods, but when we investigate more closely the difficulties are more serious than in the case of sun-spots. The following statement by Mr. John A. Fleming, acting director of the Department of Research in Terrestrial Magnetism of the Carnegie Institution of Washington, in regard to potential gradient brings out a very important difference from terrestrial magnetism, whose diurnal variation is directly related to the position of the sun:

The electrical pressure in the atmosphere results from the electric charge which exists upon the earth. The discovery of the source of maintenance of the charge is an important objective in research in atmospheric electricity. The average pressure-difference between the earth's surface and a point one meter above it is about 130 volts at sea-level.

On a "normal" day (see Fig. 10), defined as one with cloudless sky, moderate and steady breeze, clear air and high visibility, the electrical pressure varies over a small range and progresses gradually and slowly. The continuous photographic records of this "normal"-day variation obtained thus far during Cruise VII indicate again that its maximum and minimum occur simultaneously over the whole earth. Fig. 10 shows the mean diurnal-variation curve obtained from photographic records on twenty-one days made aboard the *Carnegie* during eight days, August 11 to 24, nine days, September 5 to 16, and four days, October 5 to 8, 1928, while the vessel was in the north Atlantic Ocean. These data apply for mean latitude 18° north and longitude 308° east. They are plotted in Fig. 10 according to local mean time; if referred to Greenwich mean time, it will be noted that the maximum is at about twenty hours, thus in agreement with the conclusions resulting from the earlier work of the *Carnegie*.

On a "disturbed" day, defined as one with low clouds in a partly cloudy or overcast sky, with a complete calm or a high wind bringing with it sand, dust or smoke, or with mist, fog, hail, rain, thunder, lightning or snow, there are frequent and rapid departures covering large ranges from the "normal" variation; the record for such a day is shown by Fig. 11 made

on the *Carnegie*, September 1, 1928, latitude 9° north, longitude 36° west.

Fig. 12 shows the simultaneous predominant twenty-four-hour wave as obtained on board the *Carnegie* over all oceans during her cruises 1915–21, and by the *Maud* expedition, "Drift-ice and Arctic-coast Observations in the Three Winters 1922 to 1925" (north of Siberian coast and off Siberian coast).

Earth currents which sometimes become known at times of great magnetic storms, when they may seriously interfere with telegraphic communication, are usually small but always present. They have certain, but only partial, correlations with terrestrial magnetism.

We now bring into the picture one of the most striking of all natural phenomena, the aurora. The magnetician does not look upon the aurora as a display but as a scientific phenomenon, not only in itself to be explained but to be used as a key to explain other phenomena. Its exact nature is not yet known, but it evidently is a form of penetrating radiation from outside the earth into the upper atmosphere where it becomes visible. It is probably the atmosphere that prevents the aurora from reaching the earth. Accurate measurements by Stormer and others in Norway and elsewhere, made by photographing the aurora against the stars at two places simultaneously, show that for the forms which approach closest, the lower edge is usually on the order of one hundred kilometers from the earth.

In the zones of maximum auroral frequency in the Arctic regions displays may or may not be accompanied by marked magnetic disturbances. When the displays extend far toward the equator they are invariably accompanied by

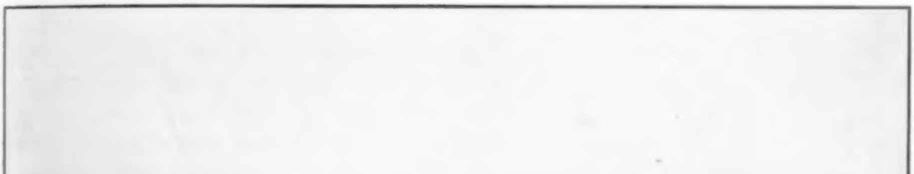


FIG. 8. EARTH INDUCTOR AND GALVANOMETER

FOR USE IN DETERMINATION OF MAGNETIC DIP IN THE FIELD.

magnetic storms and usually by strong earth currents. Here we have well-determined facts with which to test theories.

Further progress in the study of the interrelations of terrestrial magnetism, atmospheric electricity, earth currents, the aurora and solar phenomena will depend somewhat on the extent to which it will be possible to make simultaneous observations of two or more of the phenomena at the same place. There is not yet as much of this as there should be, but there is a pronounced trend in this direction. Such activities within or originating within the United States include: Magnetism, atmospheric electricity and solar observations at Tucson, Arizona (U. S. C. and G. S.; C. I. W.; Desert Sanitarium, University of Arizona); intensified solar observations at times of magnetic storms (Mount Wilson Solar Observatory); auroral observations at Fairbanks, Alaska, with magnetic observations at Sitka (Alaska Agricultural College and School of Mines, Rockefeller Foundation, C. I. W., U. S. C. and G. S.); magnetism, atmospheric electricity and earth currents (C. I. W. observatories in Peru and Australia); magnetism, atmospheric electricity and aurora, Antarctic continent (Byrd expedition, C. I. W. cooperating). This last is of unusual interest because of nearness to the magnetic pole.

We now come to a field where the correlations are of unusual interest and importance and one where the element of mystery and as yet unsatisfactory solution is comparable to magnetism—the field of radio transmission. Radio transmission around the earth is accounted for by the hypothesis of what is generally known as the Kennelly-Heaviside layer, an ionized layer far above the earth which serves to reflect and refract the radio waves so that not all the energy can pass off into space. Studies made by

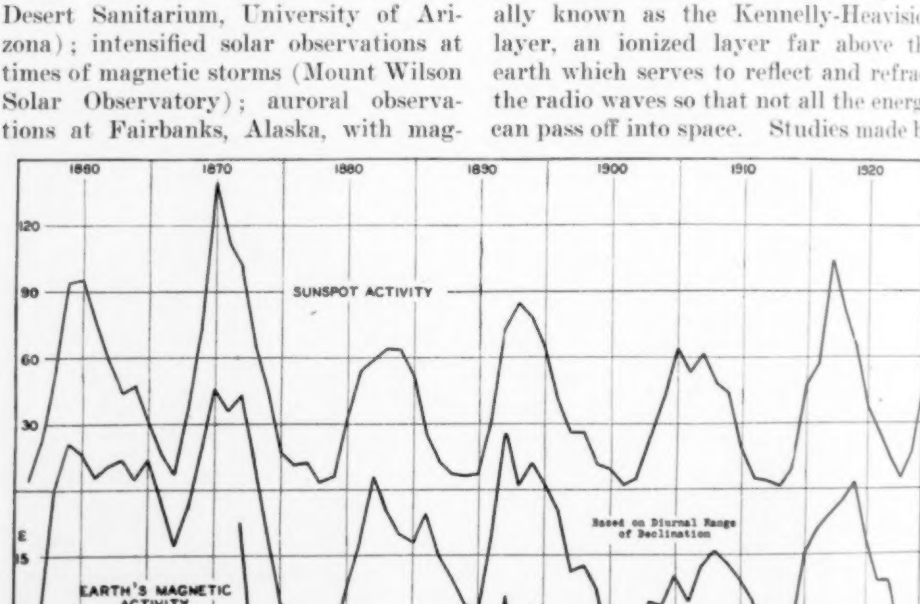



FIG. 9. COMPARISON OF SUN-SPOT NUMBERS AND MAGNETIC ACTIVITY.
*Terr. Mag. and Atmos. Elect.*, MARCH, 1926.

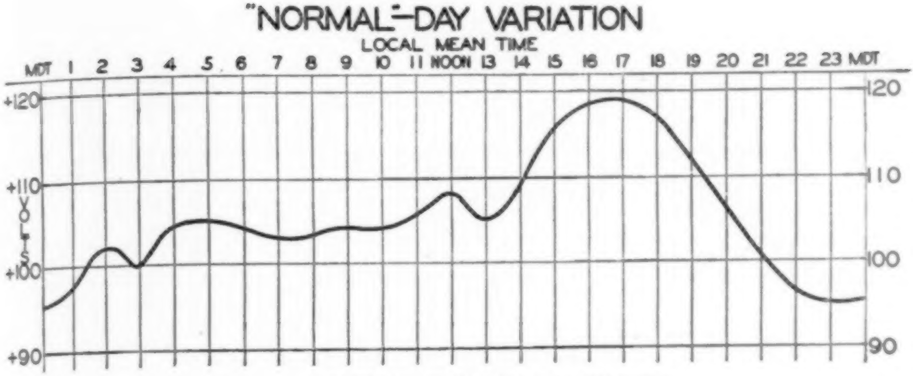



FIG. 10. NORMAL DIURNAL VARIATION
OF POTENTIAL GRADIENT OF THE EARTH'S ATMOSPHERE.

various physicists indicate that the conditions are very complex. The reflection is not ordinarily of the clear and sharply defined order of that of light, though it is definite enough for determination of the height of the layer by recording the time difference between the direct wave and that reflected from the layer. The height of the layer has been found to be different at different times of the day, and sometimes it apparently fades out at one height and comes in at another with some overlap. It is generally accepted that the phenomena of fading and skip distance are due to conditions in this layer which cause interference in the waves. Important work along these lines has been done by the Naval Research Laboratory, Carnegie Institution, General Electric Company and others.

The interesting and important fact is that such regularity of behavior as the layer may have is completely upset by a magnetic storm. In general, the height during a storm is much greater than normal and this has the result of changing skip distance conditions, that is, stations normally not receivable under given conditions come in strong while others ordinarily easy to get become impossible. The statement has been made by one of the investigators that with accurate measurements of radio reception it becomes possible to determine the magnetic character of the day almost as well as from the magnetic records. Plans are being developed by a committee of the International Scientific Radio Union for daily broadcasts from the United States of magnetic, atmospheric electric and solar conditions to aid the investigators of these problems, and similar broadcasts are already being made in Europe.

There are many lines of attack on the broad problem which can not be described. Spectroscopy is being found helpful in the study of the aurora and in deducing conditions in the upper atmosphere. The characteristic green line in the aurora has been accounted for and has been found to be present in the night sky everywhere. New ideas are being tried out which, though as yet without definite result, may at any time produce results of fundamental importance.

Many theories have been advanced to explain terrestrial magnetism, but it is impossible in a limited space even to enumerate them. An important effort of considerable promise is being made in the United States in the field of photoelectricity and ultra-violet radiation. Among the workers in this field are

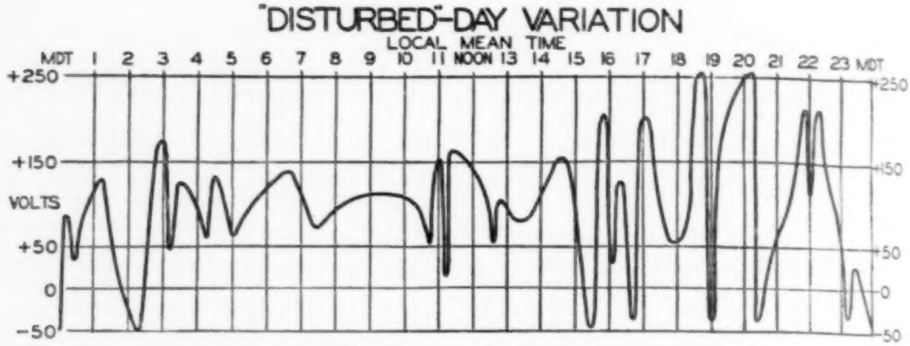



FIG. 11. VARIATION OF POTENTIAL GRADIENT
OF THE EARTH'S ATMOSPHERE ON A DISTURBED DAY.

Messrs. E. O. Hulburt and Ross Gunn, of the Naval Research Laboratory.

This theory in a general way is that all the phenomena are produced by ionization of the upper atmosphere by ultra-violet radiation of the sun. Normal radiation is associated with normal conditions, and the ionization of the upper atmosphere sets up a system of currents which are capable of producing results which agree in a general way with observations. Abnormal conditions, such as magnetic storms, wide-spread aurora and increased earth currents are, according to this theory, associated with flares of ultra-violet radiation. These flares produce an unusual number of ions in the upper atmosphere and their interaction with ions present sets up a special system of currents which seem competent to produce the effects observed.

The statement has recently been made that this theory gives values for that part of the magnetism which comes from outside the earth which are in good agreement with those already mentioned as resulting from mathematical analysis of magnetic observations.

From past experience it must be expected that many theories will be advanced before one is found which will fit all the facts. There must be a process of development of correlations, test of theories and rejection or acceptance in whole or in part, and each attempt will advance knowledge.

From the commercial view-point perhaps the most important of all applications of terrestrial magnetism is the study of the upper layers of the earth's crust by magnetic methods. There is no part of the earth where the magnetic distribution is absolutely uniform over large areas, but for most of the earth there is some approximation to uniformity. However, many parts of the earth are magnetized locally, and resulting irregularity of distribution may vary from small amounts to wide-spread magnetic fields which do not at all resemble the normal field. Certain minerals, notably magnetite, and all volcanic materials are magnetic. There is an interesting situation on the island of Oahu, Hawaiian Islands, where there are only a few places where the magnetic observations do not vary with slight change of position of the instruments because of the strong magnetization of the volcanic rock. There are, however, a number of places where, since the volcanic rock is overlaid with beds of coral, the distribution for a limited area is reasonably uniform, but when the values obtained at these places are plotted on a map it is seen that they do not fit into any scheme of uniform distribution. The magnetic method of geophysical prospecting is in effect the

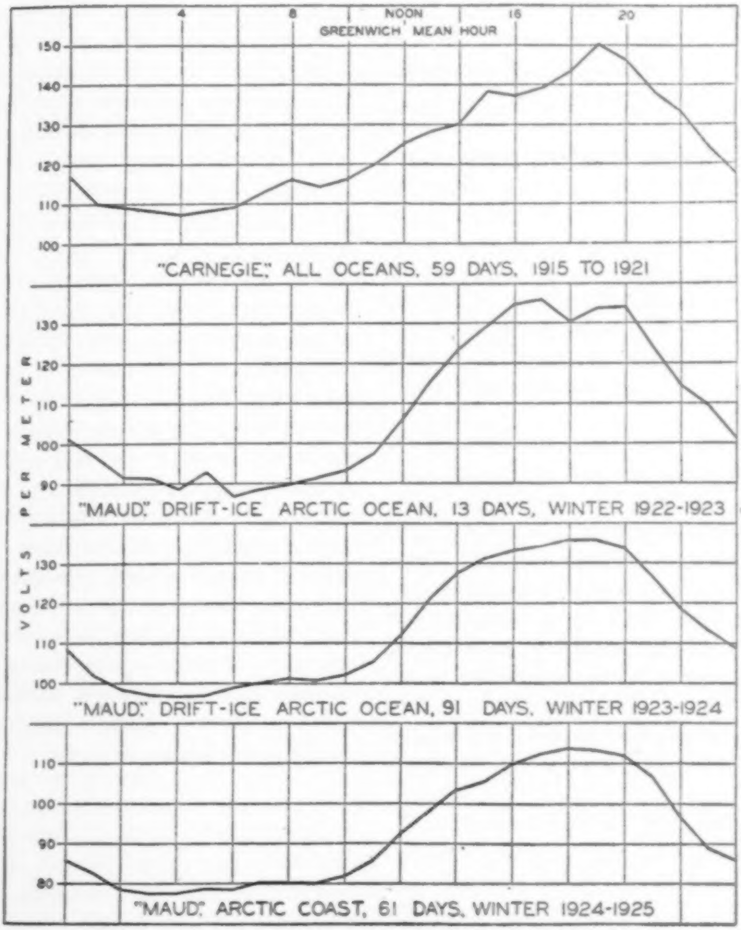



FIG. 12. VARIATION OF POTENTIAL GRADIENT
OF THE EARTH'S ATMOSPHERE FOR DIFFERENT LOCALITIES.

determination of local differences from the earth's field as measured at a reasonably undisturbed place in the vicinity. The most successful results have been in the case of magnetite and in tracing formation known to be associated with oil. The magnetic method has its limitations, but it is very generally used as it is the least expensive and so lends itself to preliminary examination of an area of considerable size. Indications found by this method are frequently developed in detail by other methods.

I have indicated that, as a result of the use of the earth's magnetism in navigation of sea and air and in surveying, accurate observations have been made which serve as a basis for investigation of the nature and cause of the phenomenon. There are other phenomena which are directly associated with it and which may be part of one great system. All these different phenomena are being studied separately and together, but in spite of the best efforts of mathematicians and physicists, and in spite of use of all the resources of modern physics, we are still at the threshold

of knowledge, and are yet far from complete understanding and ability to predict future conditions with accuracy.

In order to understand the laws—and there must be definite laws—there must be long-continued accurate observations, preferably of several of the phenomena at the same place. The study of radio transmission has proved that this is intimately related with terrestrial magnetism, and at the same time the necessity of solving the radio problems helps to provide the impetus needed and a new means of attack for the magnetic problem.

Engineers are not accustomed to undertake problems for which the complete solution is so far ahead; they like to see definite prospect of successful outcome of an investigation. It seems to me that this is assured unless the changes which have been discussed are the result of what might be called solar weather, in which case prediction is not likely to become very accurate. It may be that a point will be reached beyond which it will be impossible to penetrate, but no magnetician is willing to acknowledge that this can be the case.

Except in the case of radio, the investigations thus far made have served most of the practical needs, though prediction would be extremely useful and saving in cost. I wish to call attention again to the fact that the records obtained for known practical purposes have proved useful in two new fields of even greater importance from the commercial viewpoint, geophysical prospecting and radio transmission. Engineering research is well established on the basis of immediate or early financial return. Just as in many other fields of physics, it has been shown in the field of terrestrial magnetism that investigations started for a definite purpose have produced wholly unanticipated results. The time will come sooner or later when engineers will have to engage in research which has no immediate prospect of financial return, and they are not unlikely to find that they will make discoveries that bring even richer return.

# ANTARCTICA[1]

By Dr. ISAIAH BOWMAN

DIRECTOR OF THE AMERICAN GEOGRAPHICAL SOCIETY, NEW YORK, N. Y.

## I

An invitation from the American Philosophical Society is a command. As a member of this oldest learned society in the United States it is with great pleasure and a sense of high obligation that I address you upon invitation of our president. While the new hall will provide safer quarters for the future you will permit me to express heightened pleasure in speaking in the old hall built 141 years ago with the money raised by Benjamin Franklin and his fellow members and business friends, and thus to meet with you among these priceless scientific and historical possessions contributed in the past by an extraordinarily distinguished body of men that includes the names of George Washington, Thomas Jefferson, Charles Darwin, Theodore Roosevelt, Andrew Carnegie and Woodrow Wilson.

Among a membership pledged to "the mutual communication of their discoveries," as the original charter of 1780 puts it, there can not fail to be great interest in the Antarctic Continent, the seventh and last to be explored. That interest is heightened no doubt by the defiance with which Antarctic elements obstruct the inquisitive spirit of man. In the magnitude of the forces of ice and wind and encircling water and that "sullen barrier" of pack-ice that guards the outer seas of Antarctica, it is as if we had a transplantation of some great cosmic agency that has wrought a continent of incredible inhospitality at the South Pole as a symbol of outer worlds of mystery beyond the reach of man.

This is not fancy merely, but rather the result of sober scientific reasoning. Reflecting on the great contrast between the north and south polar regions, the one a hollow, the other a hump, Chamberlin speculated on the possibility that we have in these and other lineaments of our earth actual birthmarks, as we may call them. He saw in the assembly of materials of which the earth is composed traces of the original bolt of matter shot out from the sun to make the infant earth. The core was built up of heavier material at the end originally toward the sun and of lighter material at the end away from the sun. The heavier Antarctic end was further shaped into the southern hemisphere while the lighter materials became the ring of land that lies chiefly in the northern hemisphere. This might be called the ring of life because it made that wide belt of dry land upon which the higher types of life emerged that reached their climax in civilized mankind.

I can imagine Franklin asking Chamberlin why it is that science is so greatly interested in the Antarctic. And I can see Chamberlin pointing to the two unlike ends of the earth. "Here," he would say, "is the north-polar hollow which an echo depth-sounding made in 1927 by Captain Sir Hubert Wilkins, at a point 500 miles northwest of Point

[1] An address before the American Philosophical Society in Philadelphia, February 7. The first part of the address was broadcast by Dr. Bowman and transmitted by short wave to the three explorers now in the Antarctic. That same evening the following radiogram was received by Dr. Bowman at the Philosophical Hall from Admiral Byrd *via* New York *Times:*

"Heard most of your talk quite clearly and enjoyed it immensely. Best wishes to you old fellow from myself and all the gang. Have radioed direct to president American Philosophical Society about invitation to speak. [Signed] Dick Byrd, Little America."

The second part of the address was illustrated with wall maps and lantern slides.

Barrow, Alaska, showed to be over 17,000 feet deep; while at the south-polar end you see plainly a huge protrusion, the Antarctic Continent, bearing mountain ranges over 10,000 feet high which rise from a platform itself 15,000 feet above a ring of encircling ocean deeps."

The life of the two ends of the earth is as unlike as the topography. The ring of land about the Arctic Basin has yielded fossils in comparative abundance and, together with the abundance and variety of the plants and animals of to-day, makes possible rather definite conclusions about the migration of species from continent to continent in the subarctic zone. No such definite conclusions can be formed in the Antarctic. Exploration must go much farther before the Antarctic chapter can be written. Indeed, Antarctica is still so great a mystery that were a fossil marsupial to be discovered there the event would excite scientists probably as much as direct radio communication with Mars.

Were Benjamin Franklin, the founder of the American Philosophical Society, to hear such a conclusion he would never ask, "What is the good of exploration?" In fact, he could not have raised doubts about the value of inquisitiveness in any form under any circumstances. When a scoffer, knowing Franklin's interest in balloons, asked, "What good are they?" he answered, "Of what good is a new-born babe?" and then went on to contribute money to aeronautical experiments, never missing a balloon ascension if he could manage to attend. The man who sent his kite into the clouds to ask questions of the lightning was one of the founders of what we might call the brotherhood of curiosity. Its members want to know nothing less than all that there is to know about this amazing universe.

It is that driving curiosity about scientific problems still unsolved that has moved Byrd and Wilkins and Mawson to match Antarctic defiance with calculating skill and courage. It was curiosity, paired with technical training, that led to the development of radio communication and, as a result of that curiosity, it is possible to stand here and be heard by Byrd, while at the same time Wilkins and Mawson, thousands of miles away, may hear the same message.

Admiral Byrd, I have been asked by President Dercum to send you a personal message from the American Philosophical Society. He has asked me to say how deeply moved we all are by the reports of your expedition, how interested in its success, how anxious over its safe return. You have gained results not of scientific worth merely but of deep interest to all mankind. President Dercum has asked me to invite you to address the society upon your return that the officers and members may hear from you directly. What I have to say in the address that follows this brief radio introduction will be chiefly through the medium of twenty or thirty maps that summarize the results of your expedition and those of Wilkins and Mawson. You have no conception how personal to each of us your affairs have become. Morning, noon and night we wonder about the state of the ice—will it open in time, can the *City of New York* get through? When that ship was about to sail from New Zealand a few weeks ago there must have been many who recalled Arthur Colton's lines:

> And one ship in port to-day
> On the morrow
> Southward bound will far away
> The swift sea furrow;
> Whom the loud Antarctic waits
> And frozen citadels with creaking gates.

Our invitation is no less warm to Sir Hubert Wilkins and Sir Douglas Mawson. Through special arrangements this message goes directly to you both and as a personal friend of both I want to say with what deep admiration I follow your fortunes and wish your gallant enter-

prises success. I know that Admiral Byrd shares and often expresses equal admiration. And when the time comes for you to communicate your results to this learned society I feel sure that the admiral will be here to lead the discussion. It would gratify this assembly if within the next few days we might have acceptances to these three invitations to meet in this old hall and hear "the mutual communication of your discoveries."

All three of you have brought the Antarctic to the clubs, the schoolrooms, the family firesides of millions the world around. Would Gould succeed in finding the coal layer he was after? The question got to be a poignant one! Would Wilkins find the base he sought? Where would the ice halt Mawson? We have found ourselves involved in an "Antarctic series," with this curious condition, that everybody is betting on the success of all the teams, the three we have mentioned and the Norwegians, particularly our old friends, Major Isachsen and Captain Riiser-Larsen. This wide interest we owe not only to yourselves but to the newspapers, and the time has come when science as well as the general public should acknowledge its indebtedness to the press. Without the assistance of the newspapers the well-equipped expeditions of recent years could not have been undertaken. However efficient aeroplanes may be, they are expensive things. Moreover, we are far more interested in an expedition from which we can have almost daily radio reports than we are in one that vanishes for several years, returns with news that blazes for a week and then drops into the gulf of forgetfulness. But for the extraordinary support of the New York *Times,* which includes a triumph in radio communication, Byrd's expedition would have been impossible and public interest in it short-lived. The *Times* has contributed in this way not merely to public interest, for which it has no doubt already gained some measure of reward; it has gone far beyond this and made a contribution to science as substantial as many a scientific institution. In the same spirit the Hearst newspapers have supported Wilkins, helping him with his equipment, maintaining radio communication and assisting in the publication of his results. This group is also supporting the expedition of Sir Douglas Mawson. It is but fair that we take a moment from the consideration of the scientific results of three major Antarctic expeditions to praise these two newspaper agencies for the public-spirited way in which they have gone beyond commercial objectives and made direct contributions to scientific discovery.

But if science is to reap the full benefit of the opportunities thus thrust into its hands the scientific side of the story should be as well presented as the popular side. To this end the American Geographical Society of New York has for some years attempted the constructive task of publishing a series of books and maps of direct value to the explorer in the field as well as to scientists and laymen at home. It is extraordinary that this has not been done in the past quarter century. The explorer in the field has had to turn from one map to another. In a series of Arctic and Antarctic maps on different scales the society has brought together original surveys and meteorological and magnetic data so that the field operations might be planned upon a large base map. Our bathymetric map of the Antarctic is the only one of its kind in existence that shows the present status of our knowledge of Antarctic submarine relief on an adequate scale.

At the same time the society prepared two volumes which expedition leaders have described as an explorer's bible—"Problems of Polar Research" by thirty

contributors, among whom are included practically all the leading explorers of to-day, and the "Geography of the Polar Regions," which describes the Arctic and Antarctic region by region. These books and maps pool the knowledge of all polar explorers. They have been put into the hands of trained men on each of the present Antarctic expeditions and have led to the search for particular things, not just anything. Modern large-scale exploration, to be worth its cost, must be scientific, not merely a blind and adventurous wandering into the unknown.

If we look at the Antarctic to-day through the eyes of those who have specialized in its interpretation we shall see the real importance of the present-day search for facts. While there are over four hundred species of flowering plants in the Arctic, some of them luxuriant, there are but two in the Antarctic, and these have a precarious hold at the extreme limit of their range. Both are found on the western side of "Graham Land." One is a grass; the other represents a family of herbs. Both are dwarfed. There are no ferns in the Antarctic. Only local tundras (of mosses and lichens) up to a half acre or more are known. The short summer and the remarkably low temperature (no month has a mean temperature above freezing) is responsible for this poverty of plant life. By contrast, during the warmest part of the Arctic summer the mean temperature is above freezing, and the ground may thaw a few inches in a very few days. From these few indications you see that the present life of the Antarctic is too meager to furnish a basis for wide-ranging speculation concerning past migrations between Africa and South America or between Australia and Africa. The search for fossils is therefore keener than ever because it is chiefly through them that explanations are sought of present distributions of life by migration across the Antarctic region. These are not merely the abstractions of the specialist. The layman can get the sense of it. Did it ever occur to you how remarkable it was a year or more ago to have a whole column in a New York newspaper devoted to the finding of pre-Cambrian fossils by Sir Edgeworth David, of the University of Sydney, Australia?

It is in the rocks of Antarctica that we find the greatest realm of wonders. In earlier periods the life was far more abundant. The whole earth, Arctic and Antarctic included, enjoyed a mild climate in what geologists call the Jurassic period. At that time there lived in West Antarctica the Sequoia, the Araucaria and the Beech. We know this through fossils brought back by explorers. The former range of animals as well as plants is determined in the same way. The value of such records is so great that the search for fossil-bearing rocks in Antarctica has now become one of the major sports of science. That is why Professor Gould attached so much importance to the carbonaceous material in the sandstone from Mount Fridtjof Nansen. Even the contents of the stomachs of seals and penguins have been searched for rock specimens and thus much valuable information has been found about coast-lines where no exposed rock exists. The dredge has brought up from the bottom of the Weddell Sea at a depth of 10,000 feet fragments of Cambrian rocks, and the ice tongues that pour out through the mountain passageways about the Ross depression have borne from distant points limestone fragments of the same age that throw light upon past conditions in Antarctica.

It is the higher forms of life that interest us most, for here we are closer to the background of man himself. Where we now have a highly specialized group of birds of which three are exclusively Antarctic species—the skua, the Adélie penguin and the emperor penguin—

there was once a greater variety. If we include fossil penguins of wider range the number of extinct species already known rises to twelve. It has been suggested that the diminished number of species and their higher specialization to-day were brought about by the advancing ice in a period of glaciation even more severe than the present one, for the ice at the present time has withdrawn from the wider limits it once claimed. If Antarctica to-day seems almost buried in ice, we can only say that in a still earlier period it was overwhelmed.

Many people must wonder why it takes so long to plan a polar expedition. The matter would be simple if the explorer faced perfectly definite conditions of climate and ice. Both pack-ice and weather are never the same two seasons in succession. In spite of all the studies that have been made upon the pack-ice and all the temporary passages that have been traversed we still have very little knowledge about its behavior over substantial periods of time. It presents new problems every year. Admiral Byrd's low-powered ships were able to get through it without special difficulties when the party was deposited at Little America last year, though there were risks and trials enough. This year it is exceptionally heavy and presents a barrier three hundred miles across. Ice of that sort has to be humored, as the sailor puts it. All eyes are upon the month of February. It is then that there is the best chance of penetrating the pack and coming out again quickly. By the middle of March heavy freezing begins again. In varying width, and with breaks here and there, the pack-ice almost encircles the Antarctic Continent, in some places standing offshore for some distance, in others continuous with the ice foot of the land or the discharging ice-cap. Almost everywhere it has to be reckoned with. Mawson faces the problem near Enderby Land to-day. A few weeks ago Wilkins was held off from the land 150 miles north of Charcot Island. Riiser-Larsen, operating near Enderby Land, was able to fly to the edge of the shore ice and make his way to land on skiis, taking possession of a bit of land in the name of Norway.

The American Philosophical Society has as its main purpose "the promotion of useful knowledge." You may wonder what these scientific questions have to do with practical affairs. Of course every one knows that the Antarctic is a great source of whale-oil (used almost exclusively in the making of soap), and we know how extensive that industry has become. Most fortunate it is for the cause of scientific discovery that it pays to catch in Antarctic waters, even if the whalers have been inclined to keep their discoveries hidden as a commercial secret. They have made whale-oil pay for knowledge, combining real exploration (and great hospitality to other explorers) with economic exploitation. This is quite in the tradition of polar exploration. Old whaling captains will still tell you of that knight errant of Arctic whalers, William Scoresby, commander on thirty voyages, who chose the coast of Greenland in preference to Spitzbergen not only because he sought whales but because he had the hope "of making researches on a coast that was almost entirely unknown." He prefaces his surveys with the assurance that "an excellent cargo of whales . . . was obtained," but geography records an indebtedness greater than that of the whale-oil industry in the name "Sçoresby Sound" permanently affixed to the map of Greenland.

Whaling itself can not be more than a minor industry at best. Are there larger utilities to be sought in Antarctica? If we ask the question of agriculture in the southern hemisphere the answer is in the affirmative. It would pay handsomely in crops and cattle and security of life if meteorological stations were set up on the borders of the Ant-

arctic and in the island groups that girdle it. If we knew the habit of the "spells" of Antarctic weather there is little doubt that we should be able to find a connection between it and the rainfall and drought periods in the cereal and pastoral lands of Australia, South Africa and Argentina. It is under the impulse of this idea that Captain Sir Hubert Wilkins has carried on his explorations in the Antarctic Archipelago for two seasons. He is not down there just for fun; he is searching for suitable bases for meteorological stations to be established by international cooperation. With a ring of such stations about the Antarctic and with daily radio reports as to the weather, it would be possible to draw charts that would trace the effects of cyclones and anticyclones as they move forward from breeding places out over the southern ocean.

To forecast seasons of drought would be a practical achievement of the highest order, and no less important would it be to forecast seasons of exceptional rain. We have in Australia, Argentina and South Africa great areas of marginal lands where for several years on end it may be too dry to maintain flocks and herds and crops of normal extent. Even in years of sufficient rain the farmer needs warning to enable him to take advantage of nature's bounty. It is not putting the case too strongly to say that the practical benefits of meteorological studies in the Antarctic through the medium of a chain of weather stations outweigh all other Antarctic interests put together. We have had international cooperation for such purposes in both the Arctic and the Antarctic, the former nearly fifty years ago, the latter twenty to twenty-five years ago. But these were periods when we knew far less of the connection between the weather of high latitudes and that of the more habitable lands in which most of us dwell. From both the scientific and practical standpoints it is imperative that such stations be established at the earliest possible moment if we are fully to occupy those pioneer lands of three continents where the risk of loss still holds back settlement or puts a cruel strain upon it.

However briefly I have sketched the scientific problems of polar lands I hope that you see in polar exploration not adventure merely but serious business pursued for definite reasons by those rare men who combine physical courage and strength with scientific imagination.

## II

Exploration is somewhat like a military campaign. This is especially true in a territory so formidable as the Antarctic in size and in climatic severity. The first thing is to make a reconnoitering attack to get the outlines of the situation. It may seem like a very simple object, that of going out to map the border of the Antarctic Continent, but until this is done no one may safely conduct a long campaign in the interior. It was wise planning for Byrd to base his flights on the Bay of Whales at the edge of the Ross Shelf Ice which placed him more closely to the Pole than he could get elsewhere by ship and where he met conditions fairly well known from previous explorations. In a similar way Wilkins based his operations on Deception Island where he could depend upon the help of the whalers and from which he could explore that long finger of Antarctica which stretches up toward South America. For two seasons he has been searching for a base as reliable as Little America from which he could take off with a full load for the more than two-thousand-mile journey to the Ross Sea.

In geography we attach very great importance to this first stage. When the outlines of the continent have been mapped and the position of the offshore islands determined, when we have photographs of the coastal features and

know the habits of the pack-ice that all but surrounds the continent, when we know the location and width of the shelf ice that fringes the shore in places, when the habit of the winds is better known—then the more intensive studies at critically selected points will be vastly more profitable.

Right here we have a most important fact in polar work, that the pack-ice is never the same two seasons in succession, because the temperature varies, the force of the wind is variable from year to year and there are corresponding effects upon the currents. Very much useful work remains to be done upon the pack-ice itself. Until we can establish its habits we can not plan wisely with respect to the critical points of attack in the course of future expeditions to the still unknown sectors of Antarctica.

We take aeroplane and radio so much as a matter of course that it is only occasionally that we realize what they mean in speeding up the reconnoitering stage of polar exploration. Until November 16, 1928, no aeroplane had been flown in the Antarctic. On that day Wilkins made his first flight from Deception Island. Compasses and other instruments have been immeasurably improved. Sounding devices are now in course of construction for operation from a speeding airship. Canned heat as well as food can be carried. There need be no fear of starvation or scurvy. Byrd timed his flights along a meridian in a short period of clear weather on both his north and south polar expeditions. A vast amount of work can be done with present-day instruments in a single spell of good weather. Wilkins' flight over "Graham Land" was likewise timed by the weather. On his great flight from Alaska to Spitsbergen, he spotted a storm far ahead and flew around it, thus maintaining his visibility and making certain of his position. The radio enables one to learn the precise time and has become an important element in position finding besides vastly increasing the security of the polar explorer. He can be in constant communication with sources of information at home. On several occasions the American Geographical Society has been requested to supply, by radio, technical information concerning maps and soundings for the Byrd expedition, as it did for Wilkins before his Barrow-Spitsbergen flight. As new conditions arise the explorer in the field can now call upon the scientist at home for advice and assistance. He can command resources as quickly as if he had the advantages of a post office or a telegraph station at hand. The possibilities of such collaboration in the future are very great, since the explorer in the field can obtain information and advice which were totally inaccessible to his predecessors.

Unquestionably the most important information in the reconnaissance work that represents the first stage of exploration is gathered by the camera operated from the aeroplane. By this means there have been outlined in two seasons by Wilkins practically the whole of the hitherto unknown portion of the Antarctic Archipelago and the coast of Hearst Land for several hundred miles. Byrd has photographed a stretch between Little America and the Pole and has added a large piece of territory east and northeast of Little America which includes a newly discovered mountain range, as well as Marie Byrd Land and the Rockefeller Mountains discovered last year. When these photographs are built into a mosaic and reduced to a map we shall have substantially a topographic base map from which later operations may be planned into the still undiscovered country on the east.

Every subsequent explorer will have his way made easier by such precise information. In addition, we shall be able to see the relation of mountain to mountain and build up those hypotheses

which will lead to better planning in the search for critical information that still remains to be found before we can complete our picture of the form and build of the Antarctic Continent. This is the way of science. A new discovery is not an end to anything. It is a step in an unhalting advance that began when speech and fire and the spear and the floating log were no longer mysteries to awakening man.

Broadly speaking, there are two areas of greatest interest from the standpoint of reconnaissance work. The first is the tracing of the depressions of the Ross Sea and the Weddell Sea to their heads in order to determine if there is a salt-water connection right across Antarctica, thus making a great island of the territory between Little America and Hearst Land, or possibly a group of islands. Both Byrd and Wilkins hoped to carry their flights inland far enough to determine this point, which was among their announced objectives. But neither has succeeded in doing more than continue the probability that these two depressions run farther inland than we had been led to expect. Most significant is the discovery of Gould that the Ross Shelf Ice extends at little elevation above sea-level, past the 150th meridian, and swings eastward along the front of the Queen Maud Range farther than we had been led to expect from the reports of Amundsen, whose Carmen Land now drops out of the map. Even if a salt-water connection were found to exist it is altogether probable from the slight elevations already determined at the heads of these depressions and from the soundings on the border that the connection is ice-filled throughout. And if an archipelago exists to the northward of this depression the separate land units are probably "welded" together, to use Mawson's term, into a single unit continuous with the larger part of the continent bisected by the 70th meridian east of Greenwich.

Two great objectives are the determination of the position of the coast-line between Enderby Land and Coats Land and from Hearst Land to King Edward VII Land. The flights of Byrd and Wilkins have cut off six hundred miles of hitherto unknown coast along the southern Pacific Ocean, reducing to 1,600 miles the unexplored section. The Coats Land-Enderby Land coast is of equal length and is succeeded by another stretch half as long east of the 50th meridian.

In the field season 1928–29, after establishing himself at his camp at Little America, Byrd undertook a flight to the northeast as far as Alexandra Mountains, from which he flew south to discover a new range which he called the Rockefeller Mountains. On a second flight past the southern end of the Rockefeller Mountains he discovered high land beyond the 150th meridian west which he named Marie Byrd Land. Fortunately, it was possible to make a second flight the same day with the aerial mapping camera and thus to provide the basis for a detailed map; and later on Dr. Laurence Gould, the geologist of the expedition, flew to the Rockefeller Mountains and collected rock specimens.

In the season just closing, Admiral Byrd has been able to make a number of flights of which two are of outstanding importance. A first was made on November 28 and 29, 1929, when again the aerial mapping camera was employed on an eight-hundred-mile flight to the Pole. The round trip of 1,600 miles was made in nineteen hours, including a stop of one hour at a refueling base. Of importance to the success of the flight was the radio report on the weather as far as the foot of the mountains over four hundred miles from Little America, made by Dr. Gould's geological party. In return Byrd was able to help the geological party by dropping a package of photographs taken on a previous base-

laying flight, thus enabling Gould the better to plan his advance to his distant goal, Mount Fridtjof Nansen and the Queen Maud Range. Byrd's second flight was intended to extend his discoveries toward the east of Little America while at the same time he established an independent communication with the sea-coast outside of the Ross Dependency. In this manner he was able to set up the basis of an independent claim on the part of the United States to newly discovered territory tied directly to the coast.

It was his good fortune to discover a deep salt-water entrant on this flight which seems to cut off King Edward VII Land from a new mountain range extending along the 147th meridian west, to trace one hundred miles of new sea-coast and to connect the new mountain range with Marie Byrd Land in the interior, thus providing the framework for a physiographic description of an entirely new group of topographic features. At the same time the trend of the newly discovered coast throws light upon the hitherto baffling question as to why there should be such a great concentration of ice in the Ross Sea and in the region to the northeast. The whaling operations of the Norwegians have shown the pack-ice thereabouts to extend far to the north as if it were held back by land still untraced or by groups of islands that might serve equally well to confine the ice in the direction of the Ross Sea.

The sledge party under Dr. Gould which had Mount Fridtjof Nansen as its objective was able to explore new territory and at the same time gather rock specimens from localities so close to the Pole that both rocks and fossils have exceptional value in determining the probability or improbability of a connection between the Queen Maud Range and the Antarctic Archipelago. New hypotheses of origin may thus be set up that will help direct future explorers in their search for additional information. From 6,500 feet elevation on the flank of Mount Fridtjof Nansen, Gould found a sandstone that includes a layer of carbonaceous material, thus extending to this distant interior point the so-called Beacon sandstone found at other points in the long stretch of South Victoria Land on the west side of the Ross Sea, and making it more probable that the south polar plateau, east of the 160th meridian east, is underlain by a vast coal field.

Wilkins' great flight from Deception Island, six hundred miles south, on a meridional course, is one of the great feats of exploration by aeroplane in the Antarctic. In five and a half hours of outward flying he transformed our whole conception of so-called "Graham Land," proving that it was broken into separate units and that as a whole it was separated from the main body of the Antarctic Continent by scattered islands and a water passage filled with ice. The name "Graham Land" thus drops off the map and the whole group of islands may more properly be designated the Antarctic Archipelago. Through his photographs from the air and the positions indicated on his route it was possible to construct the outlines of the eastern coasts of the main islands composing the archipelago as shown in the July, 1929, number of the *Geographical Review* and as reproduced upon the map accompanying that issue.

It was a most startling and important discovery that Sir Hubert had made, and it is a great satisfaction to record that in the present field season he was able by a short flight from a base on the western border of the archipelago to check his determinations of last year and show the substantial accuracy of the positions indicated upon the map that originally appeared in the *Geographical Review*.

In addition, he carried out a four-hour flight on December 31, 1929, which enabled him to trace what appears to be

the mainland of the Antarctic Continent westward just south of the 70th parallel as far as the 80th meridian. He took off from the water beside the *William Scoresby* at 67° 47′ south and 75° 21′ west. On this flight he proved the insular character of Charcot Island, and photographing the island and the mainland shore and observing the trend of the shore east and west he was able to connect with his discoveries of last year and establish the relative positions and relationships of the Antarctic Archipelago and mainland in a manner invaluable to future exploration and very gratifying to the cartographer.

Upon the other side of Antarctica there is an expedition of which we shall hear much more before the end of another year. It is conducted by Sir Douglas Mawson and sponsored by the governments of Great Britain, Australia and New Zealand. As on his previous expedition of 1911–14, Mawson proposes to explore that part of the Antarctic border that faces Australia as well as the section between 50° and 80° east. He hopes to make studies on the continental shelf that will be primarily oceanographic in character. His ship, Scott's old *Discovery,* is equipped with modern apparatus for oceanographic work. He expects to take a large number of soundings on the continental shelf; determine the outer border of that shelf, which is, in fact, the outer border of the continent itself; investigate the physical and biological features of the water overlying the continental shelf, and make such studies of sea organisms and sea floor as will throw light upon the origin of the continent as well as the distribution of life in a manner that may be useful to the whaling interests. He has an echo depth-sounding machine as well as a drum and cable for making soundings of the conventional type. He will secure bottom and water samples and, by means of a small scouting plane, has planned flights from the ship over the ice fringe in order to determine, if possible, the outlines of the coast hitherto unseen over much of the section that he expects to explore.

Mawson has already done important work. He has gone by way of Cape Town, Possession Island (one of the Crozet group), and thence south to a point in the ice-pack about on the 75th meridian east, being held off from the land by heavy pack-ice. The soundings becoming shallower, a flight was made in the scout plane on December 31 from 66° 11′ south and 65° 10′ east, and on this flight, beyond a forty-mile belt of unbroken ice and ten miles more of coastal water, there were sighted what appeared to be low, hilly, ice-covered tracts of land.

Mawson's concentration upon the continental shelf is in line with the most advanced geologic thought concerning the meaning of this shelf and the life found in the waters upon it, in the recent up and down movements of the Antarctic Continent and the heavier glaciation that preceded the present stage of ice retreat. Though the land life is poor, the shallow water life about the rim of the Antarctic is amazingly rich. Owing to the upwelling of the deeper waters near the shore, as the strong winds brush the surface waters away from the continent, there is brought from below the deeper oceanic waters rich in nitrogen. The coastal waters also contain an abundance of silica owing to the low temperature and to the large quantity of rock waste swept down by individual glaciers as well as the Antarctic ice-cap. There are 0.5 parts per million of nitrogen in Antarctic waters in contrast to the 0.15+ parts per million in the North Atlantic and 0.10 in tropical oceanic waters.

The high nitrogen content of Antarctic waters makes them an ideal home for those immense quantities of diatoms that

furnish the base for higher forms of life in succession. This is the key to that immense development of seals, penguins and whales that excite our curiosity by their appearance in waters adjacent to the coldest, most desolate and most terribly windswept land mass in the world, the "home of the blizzard" as Mawson called it. Mawson saw 16½ acres of penguins in Macquarie Island, half way between New Zealand and Antarctica, and it is estimated that a million penguins were observed in one rookery in the South Orkneys, in latitude 60° on the northern border of Weddel Sea.

No account of Antarctic exploration would be complete without reference to the scientific work carried out, especially in the last few years, by Norwegian whaling interests. Wishing to free themselves from the obligation to pay a license fee to the British government, the Norwegians have steadily developed larger ships capable of maintaining themselves for a season at sea in areas outside those under British jurisdiction. To help them toward an independent status they have annexed Bouvet Island southwest of Cape Town, about on the 55th parallel, and Peter I Island which lies at the edge of the pack-ice on the 90th meridian west of Greenwich, or 10° west of the western border of the Falkland Islands Dependencies. During the last three Antarctic summer seasons four expeditions have been sent out, and in the present season, 1929–30, three Norwegian vessels are in the Antarctic engaged in scientific exploration. Both Major Gunnar Isachsen and Captain Riiser-Larsen are now in the Antarctic in vessels equipped for exploration, and reports have been received of operations between Coats Land and Enderby Land, where fast land has been discovered and "possessed" for Norway. In this they were helped by scout planes, by means of which they were able to fly from their ship one hundred miles to open water from the edge of which they traveled by skiis, raising the Norwegian flag on the shore. The whalers have also skirted the pack-ice west of the Antarctic Archipelago right around to the 140th meridian west, thus making many new determinations of the position of the ice that will be of great value to science when available in published form.

# THE PASSING OF METAPHYSICAL IMMUNOLOGY

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## I

OUR first half century of modern experimental immunology has given to scientific medicine some of the most spectacular clinical victories of all times. No therapeutic triumph is greater than the miracle of diphtheria antitoxin, few diagnostic successes rival the certainty of the Widal serological test for typhoid fever, no surgical boast is more justified than serologically controlled human blood transfusion. This relatively brief period of applied biological research has been characterized by the rise and fall of a half dozen immune theories, condensing into five decades two millenniums of previous clinical evolution.

The initial immunologic theory of this period is usually credited to Pasteur. Pasteur noted that artificial cultures of pathogenic micro-organisms are usually short-lived, rarely remaining viable longer than a few days or weeks. This he assumed to be due to starvation, exhaustion of suitable food material in the culture medium. Applying this exhaustion theory to the human body, Pasteur pictured convalescent immunity as a result of a complete destruction of some necessary bacterial pabulum in human tissues. Assuming that each and every bacterial species has its own highly specialized nutritional needs, and that the necessary chemical factor or group of factors is very slowly or incompletely regenerated in human tissues, this exhaustion theory gave a logical and consistent explanation of all facts of convalescent insusceptibility known during the initial Pasteur decade. With the subsequent demonstration of successful serum transfer of convalescent immunity, however, the specific exhaustion theory became untenable. It was impossible to picture an effective serum transfer of an absent chemical component. Pasteur immunology, therefore, became the first obsolete immunology of the modern era.

## II

Pasteur's later contemporaries soon demonstrated, however, that self-limitation of artificial bacterial growth is usually not due to exhaustion of suitable food material but to accumulated bacterial products. A new concept of convalescent insusceptibility was thus suggested, which pictured acquired immunity as a result of retained microbic products or their secondary biochemical derivatives. This retention theory was championed by numerous serologists, particularly by Buchner, with whose name the specific retention theory is usually associated.

Assuming that retained microbic products or their secondary derivatives are very slowly or incompletely destroyed or eliminated from the human body, Buchner immunology was in full accord with all known serological facts of his time. It was an apparently logical corollary to the buchnerian postulate, however, that there must be some constant quantitative relationship between the injected dose of a morbidic agent and the resulting convalescent antimorbidic or protective substance. So paradoxical were the observed relationships between injected diphtheria toxin doses and the resulting serum antitoxin, however, that Buchner himself was convinced of an error in his retention hypothesis, which he, therefore, repudiated. Buchner im-

munology, therefore, became the second obsolete immunology of the modern era.

III

For its third theory, the adolescent science of immunology now turned to the real or assumed facts of tissue physiology. With minds polarized by subconscious ancestral beliefs in warring spiritual forces in the human body and in antagonistic morbidic vital humors, Pfeiffer assumed that the tissues of the first man were endowed with minute samples of specific antidotes against each and every morbidic agent the sons of Adam were predestined to meet, or that a similar plurality of intracellular antimorbidic endowments has resulted from the miracle of evolution. Pfeiffer further assumed an intracellular potentiality of increasing and flooding the human body with these hereditary specific antimorbidic humors in times of need.

For a logical theory of convalescent insusceptibility it was only necessary for him to postulate an effective physiological stimulus for this hereditary emergency internal secretion. Inspired by Pavlov's demonstration of reflex secretion of gastric juice, Pfeiffer found his endocrine stimulus in a postulated highly specialized nerve-reflex. This, of course, was equivalent to assuming a multiplicity of morbidity-recognizing sensory nerve endings in human tissues, or an equal multiplicity of morbid-specific motor nerve impulses. Neither assumption was verifiable by contemporary physiology. Pfeiffer, therefore, in turn, voluntarily sacrificed his reflex immunology on the altar of medical progress.

IV

Pfeiffer's basic faith in hereditary, polyvalent, antimorbidic endocrine endowment, however, was not discarded. Coupled with the new assumption that the immunologic stimulus is chemical rather than neurological, and that the morbidic factor itself functions as an exogenous hormone, the Pfeiffer hereditary immunologic endocrine was endorsed by Ehrlich and in time elaborated by him into what is now known as the "side-chain" theory or "specific receptor" theory of immunity. The characteristic feature of the Ehrlich theory is the assumption that the postulated, hereditary, specific, intracellular, antimorbidic humor is an integral part of the living cytoplasm, a molecular side-chain or sessile receptor, having a specific superavidity for the morbidic agent.

Specific-receptor immunology furnished a logical and consistent explanation of all known facts of immunity at the time of Ehrlich. Flooded with highly avid, desquamated side-chains, the morbidic agent was killed or neutralized in the body fluids before it had time to unite with vital cells. Serum transfer of "free receptors" readily accounted for successful diphtheria antitoxin. Test-tube agglutination of specific micro-organisms, precipitation of specific foreign proteins, laking of specific foreign erythrocites and other observed serum reactions were logical results of the assumed chemical avidity of these humoral side-chains for morbidic agents. Varying numbers, avidities and valences of humoral and sessile receptors in different animal species and in different organs and cells of the same species fully explained all known variations in hereditary or natural resistance. Within five years specific-receptor immunology became the international basis for theoretical deductions and clinical applications, which proud position it held for at least a quarter century.

Of course, Ehrlich immunology was not without its critics; but criticism was almost invariably directed against some of its minor, relatively unimportant secondary hypotheses, rather than against its basic concept. Interminable debates as to whether or not the postulated morbidic-antimorbidic avidity obeyed the new-discovered laws of electrolytic dis-

sociation. Prolonged controversies as to whether or not observed inconsistencies with the chemical law of multiple proportion were due to asymmetrically polyvalent receptors or to a plurality of closely related monovalent receptors. Difference of opinion as to whether or not the apparent cooperation of specific receptors with certain normal serum defenses is in the form of their joint attack on the morbidic agent, or a first-wave specific attack, followed by a second-wave non-specific "mop-up."

## V

During the first decade of the Ehrlich dominance very spectacular confirmatory evidence was discovered in the newly recognized phenomenon of acquired specific hypersusceptibility, the apparent exact opposite of acquired immunity. This hypersusceptibility is also transmissible by serum transfer. By postulating intracellular proliferation of specific receptors without adequate desquamation into the blood stream, increased morbidic avidity of vital cells could be pictured, without compensatory humoral protection. By assuming that desquamated side-chains are homesick for their cells of origin, and that, transferred to a normal animal, they leave the blood stream to reunite with their mother cells, serum transfer of increased morbidic avidity could be explained. Professional immunologists, however, realized that by this time they were endorsing a theory consisting of no less than twenty superimposed major and minor premises,[1] the most monumental polysyllogism of modern sciences.

While clinicians were pointing with pride to diagnostic and therapeutic successes following logical applications of the Ehrlich concepts, professional immunologists were astounded by the ever-increasing number of theoretical paradoxes and clinical failures. Every diagnostic victory was matched by a score of unpredictable diagnostic failures. Each clinically valuable vaccine was offset by twenty unexpected prophylactic or therapeutic vaccine disappointments. Every statistically valuable antiserum was accompanied by dozens of statistically inert serological placebos. With no Ehrlich-deducted clinical application were the probabilities greater than 1:20 of statistically demonstrable clinical success. Moreover, professional immunology soon found that many apparent successes were purely accidental findings, due to wholly unpredictable collateral phenomena. The Wassermann diagnostic test for syphilis is the outstanding example.

## VI

The newly recognized phenomena of specific hypersusceptibility gave laboratory immunology, for the first time, a series of easily recordable physiologic reactions with which the truth or falsity of the specific-receptor hypothesis could be tested: fatal spasmodic closing of the bronchioles, lethal cardiac paralyses, obliterating arterial vasoconstriction, fatal syncopy, explosive abortion, uncontrollable defecation, altered threshold of nerve stimulation, abolition of nerve reflexes, local edemas, changes in total blood volume and altered chemical composition of blood plasma on tissue lymph. A decade of immuno-physiological research gave such a mass of Ehrlich-irreconcilable physiological data that even the most ardent champions of specific-receptor immunology were forced to the humiliating conclusion that for three decades they had dwelt in a world of biological make-believe.[2]

With loss of faith in Ehrlich orthodoxy, there was the still more humiliating realization that for three decades immunological research had directed its main attention away from the particular biological fields whose mastery might have given adequate control of infectious

[1] "The Newer Knowledge of Bacteriology and Immunology," University of Chicago Press, 1928, p. 1098.

[2] SCIENTIFIC MONTHLY, 25: 362, 1927.

diseases. Cytophysiology, for example, the basic immunologic field opened by Metchnikoff, only to be abandoned under the Ehrlich omniexplanation of all immunologic phenomena. A dozen Jacques Loebs and a score of years can not compensate for this neglect.

Equal indifference in the immunochemical field, which was equally insignificant to Ehrlich immunology, the postulated morbidic-antimorbidic avidity being a complete explanation of all immunochemical phenomena. Outside the distribution of a few easily recognized bacteria, for example, we have to-day no knowledge whatsoever as to the immediate or final typographical distribution of a single injected morbidic factor of immunological importance. Whether such agents remain in the body fluids, are deposited on cellular-humoral interfaces or are taken up by the living cytoplasm is beyond the reach of current hypothesis. No known facts as to the relation of morbidic factors to humoral, interface and intracellular enzymes, possible hydrolyses, depolymerizations, homologous and heterologous conjugations. Only semi-theological deductions as to the probable length of stay in the human body. A generation of Hektoens, Wellses, Landsteiners and van Slykeses will be required to compensate for this neglect.

## VII

In spite of this glaring vacuum in our basic cytological and biochemical knowledge, clinical pressure is so great that a dozen immunologists have hurdled our basic biological ignorance to the tentative working hypothesis that retained biochemically "hybridized" morbidic agents are the protective "antibodies" of the Ehrlich nomenclature, a renaissance of the "obsolete," discredited immunology of Buchner. As a corollary to this hypothesis they have assumed that protective buchnerian "hybrids" can be prepared in the chemical laboratory.

Within the last five years a dozen alleged successful test-tube antibodies have been reported. By incubating mixtures of diphtheria toxin and normal horse serum, for example, Kryshanowski reports the successful test-tube synthesis of a therapeutically active diphtheria antitoxin.[3] By incubating similar mixtures of sera and plant juice Mez,[4] Sasse[5] and Nahmacher[6] report successful artificial specific precipitants for plant proteins and specific agglutinations for plant cells. In their hands these artificial buchnerian hybrids are identical with the natural antibodies following animal inoculation.

Similar successes are reported by the Czechoslovakian clinician Kabelik in his alleged synthesis of specific precipitins for animal proteins.[7] Kabelik, however, is not convinced of the identity of his products with natural precipitins. The Russian urologist Baschkirzev currently alleges successful specific buchnerian bactericides for the gonococcus.[8] It has, of course, long been known that specific agglutinins occasionally appear in artificial bacterial cultures, but their possible buchnerian significance has been overlooked.

Under the assumption that normal antibodies are probably formed as a result of enzyme action, Sdrawosmisslow[9] and Kimmelstiel[10] have incubated mixtures of diphtheria toxin and commercial trypsin, and report the formation of a "diphtheria trypsinate," therapeutically antitoxic. This, however, is not believed by Kimmelstiel to be identical with normal diphtheria antitoxin.

## VIII

Of even greater theoretical and clinical significance are Kryshanowski's

[3] *Centralb. f. Bakt.*, 110: 1, 1929.
[4] *Botanisches Archiv.*, 12: 163, 1925.
[5] *Beitrag. Biol. Pflanzen*, 16: 351, 1928.
[6] *Ibid.*, 17: 1, 1929.
[7] *Biologické Listy*, p. 31, 1927.
[8] *Zeitschr. f. Urologie*, 23: 92, 1929.
[9] *Zeitschr. f. Immunitätsf.*, 54: 1, 1927.
[10] *Ibid.*, 62: 245, 1929.

studies of buchnerian hybrids with different serum proteins.[11] While with certain proteins the test-tube hybrids are therapeutically antitoxic, with other serum fractions they are wholly inert. With two serum fractions specific supertoxins are formed, more potent than the original filtrate.

Evidence that similar supertoxins may be formed in the animal body is suggested by Torrey and Kahn's[12] recent report that certain bacterial toxins injected in the femoral bone marrow are locally "hybridized" into highly potent bone-marrow cytotoxins. Absorbed from the local marrow they cause degeneration of all bone marrows of the body, with a resulting, persistent, often fatal anemia, simulating pernicious anemia in man.

## IX

Current immunology, therefore, is essentially a revolt against Ehrlich metaphysics, with buchnerian chemistry as the most promising immediate method of attack. The buchnerian renaissance, however, is not repeating former errors. It does not assume that retained biochemically hybridized morbidic agents are the only specific antibodies formed or liberated in the animal body. Nor does the most ardent buchnerian apostle assume that variations in amount and topographical distribution of retained hybrids throw any light whatsoever on the nature of normal bodily defense.

Whatever may be the future of this renaissance immunology, its serious reconsideration at this time throws doubt on many cherished preconceptions of the last three decades. The probability that retained morbidic hybrids may vary qualitatively with time, dosage, portal of entry and topographical distribution of an injected morbidic agent, the possibility that at any one time the retention hybrids may not be identical in different organs, tissues and body fluids, seriously challenges three decades of clinical deductions with suggested vaccines, antisera and diagnostic methods. Proof of the Buchner immunology might necessitate a complete reexamination of forty years of careful and conscientious experimental and clinical work, a threatened research emergency that is officially recognized.[13]

No less disconcerting is the possible bearing of the Buchner theory of biochemical relativity on general biology. Veblen, for example, reports that certain bacteria grown in dilute horse serum are apparently hybridized with horse proteins, as shown by their acquired agglutinability with antihorse precipitins, and their loss of agglutinability with their own specific agglutinins.[14] Veblen's data suggest more radical changes than a mere mechanical formation of horse protein films about the bacteria, as previously postulated by Shibley and others from observed changes in electropotential.[15]

## X

The threatened Buchner renaissance marks the beginning of a new era in theoretical and clinical immunology, loss of faith in metaphysical cytology and biochemistry, adoption of the ideals and methods of the older biological sciences. With chastened spirit, but with undaunted hope, with undiminished faith in ultimate clinical victory, for the first time in all history immunology endorses the Hippocratean command, "Draw no deduction from anything except an assured fact."

[11] *Loc. ref.*

[12] *Amer. Jour. Path.*, 5: 117, 1929.

[13] *J. A. M. A.*, 94: 654, 1930.

[14] *Proc. Soc. Exper. Biol. and Med.*, 27: 204, 1929.

[15] *Arch. Path. and Lab. Med.*, 2: 438, 1926.

# THE IMPERSONAL OXFORD

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OXFORD is deceptive. Take, for instance, the tourists who have been awed by its moldering masonry. How many of them know that Oxford stone is perhaps the softest that has ever gone into architecture? How many of them know that an Oxford building has to be refaced about once in forty years, so that few of the stones which cause them to catch their breath are older than themselves? Of course Oxford *is* ancient. But the tourist is likely to point with awe to very modern decay. And the educator often assigns a quaint and perhaps pleasing accident as the true cause of Oxford's achievement in civilization.

Talk about Oxford in this country has been based on mistaken conceptions of the great English university. Educators have seized upon superficial features. At the same time a kind of myth has sprung up among college students—a myth in which Oxford plays the rôle of a fairy prince rescuing the student from durance vile.

The educator's view of Oxford has often been developed in a few visits to the high table and the senior common room. There is much delightful talk from the tutors, and answers to questions about pedagogical method are most casual. This tends to nourish the idea that a tutorial is ringed with tobacco smoke and incubates "personal contact." The educator has probably carried this picture with him to Oxford, so that it takes very little of the common room to stamp it in. He is probably familiar with Stephen Leacock's picture of intellectual osmosis in a tutorial: there is no activity in the tutorial except puffing at a pipe, and yet somehow at the end of four years an enormous amount of something valuable has been conveyed to the student. This view has been promulgated so widely that one is tempted to ask why the Pullman smoker has not been accepted as the school for American youth.

I know a college president who has been very zealous in the development of honors work, organized for weekly conferences of student and professor. He still thinks of Oxford in terms of the free and easy palaver of the common room where the dons gather after dinner. This educator often announces to the community which he heads that when students talk so much and so well education will be in our midst. And there is the president of Harvard University, who feels that the division into small colleges is a major factor in Oxford's success. Harvard is on the point of spending thirteen million dollars for houses which will resemble the Oxford college. Dr. Lowell seems to agree with Bagehot that education is largely the impact of youthful mind on youthful mind. We must provide the physical paraphernalia for a friendly rubbing together of a small group of students. This conception of personal contact was naturally not invented at Oxford. With us it is very largely a reaction against the monstrous size of our universities. But the reaction is often caught looking to Oxford for its next step, and it sees the advance in terms of "personal contact."

In the spring of 1924 the Harvard English department decided to adopt the tutorial system. It provided funds for two of its younger instructors to spend the summer in England, doing some research and incidentally picking up some suggestions about tutoring at Oxford and Cambridge. Since these two gentlemen had only the summer at their

disposal they found neither Oxford nor Cambridge in session. The few tutors with whom they conversed told them that there was very little to say about the tutorial. It is as obvious and natural as a meeting of two people who have work to do; one merely follows one's nose to find it. Provide an adequate faculty and an adequate interest on the part of the student and you will always have it.

Is there then nothing to be learned about the machinery of the intellectual life at Oxford? There is, but unfortunately these two very estimable representatives of Harvard were unable to get at it. They were not prepared to understand what lies behind the tutorial and makes it so powerful an instrument. There is a great deal behind its "personal contact." The English tutor does not normally attempt to deal with students whose interest has not been developed before they come to the university. He often regards the American attempt to make the tutorial an instrument for creating interest as a hopeless one, and one which distracts attention from the main problem, which is to build up interests before the student appears in the university. Without such interests already developed the sort of personal contact which one finds in an Oxford tutorial would not exist.

The Oxonians who talked to the representatives of the Harvard English department in 1924 probably listened in some astonishment to the basic ideas which the latter were outlining as the *raison d'être* of the new venture. A tutor at Harvard was to pull the threads of the various courses together and rouse interest by any kind of assignment which seemed to be on a level with the student's capacity. There were to be occasional meetings of groups of eight or ten, with a fire as a center, and all as informal as possible. And it was something of this sort which the Harvard instructors expected to find in the Oxford tutorial. Their ideas were sufficiently remote from the real basis of the tutorial as it is conceived at Oxford to make it rather difficult for an Englishman, even a willing Englishman, to answer their questions in his own sense. It would be difficult for the Englishman to indicate his profound disagreement with the informal, ultra-personal view of education without dilating on the elaborate scheme of organization of which the tutorial is the focus, and for this he rarely gets a hearing.

Naturally the Harvard English department has done a great deal of useful tutoring, since many of its tutors are intelligent. And its conception of tutoring has probably changed somewhat with increased experience. But Harvard has not been able to profit by suggestions from Oxford, because it does not understand them. One would naturally assume that suggestions from Oxford would have to be reinterpreted in the light of the difference between English and American conditions, but even that is not possible, because the difference itself is not understood in this country. In the last few years we have had in the tutorial system at Harvard and the honors courses of institutions like Swarthmore and Williams a departure from the older American methods. But we must constantly keep in mind the fact that these experiments have not brought us any nearer to Oxford, though they make use of certain methods which have a superficial resemblance to Oxford procedure. The principle of English education differs from both the new and the old in American projects.

Oxford's success is due to its assumption that the major part of a university education is to be secured at school. And it does not merely think this (many professors at Harvard and Swarthmore and Wisconsin do that) but it acts upon the assumption. English parents usually make much greater demands of a school than do American parents. Yet

even this is not counted sufficient to guarantee that freshmen will be adequately prepared. Oxford takes special measures. Each of the twenty-one colleges offers a large number of scholarships to be awarded as the result of examinations in a special field like classics or chemistry. The usual value of a scholarship is $350 to $500 per annum; they range up to $1,000, and a few in each college pay smaller sums. These scholarships are sometimes won by men who can afford to pay their own expenses and who therefore give up the emolument to a poorer competitor but retain the title because of the honor it brings with it.

When money is given to an Oxford college it usually takes the form of a scholarship to be awarded on the basis of examinations. It is not given exactly for the support of a deserving boy, but rather for the support of a deserving boy whose intelligence and training have been shown to be such as can profit by further cultivation at Oxford. This is demonstrated largely by written examinations.

A school's reputation from the scholastic point of view depends solely on the number of scholarships at Oxford and Cambridge which it is able to win. Naturally a good deal of attention is paid to sifting out the promising boys and developing them in their best subject. Though Oxford makes less noise about opening its portals to rich and poor alike, it has actually been more efficient than most American institutions in giving the poor boy an education. It assumes that there will be no use in having the poor boy in a university unless he is properly schooled. It has taken the necessary steps to see that the schools develop and train any boy who shows promise. If the school can lay its hands on a bright boy by offering scholarships, or in any other way, it will receive an immediate distinction for what it does to pick him out of the ruck and give him the necessary tools.

The school's anxiety to maintain its reputation by winning scholarships affects a great many more boys than are included among the actual winners of scholarships. Any boy who has a chance to win a scholarship will be given plenty of individual attention, and in his last year or two will probably be released from a good deal of the school routine to read intensively on his own, and write essays for criticism by a master.

"Write essays"—this looks like a program in English which would be of very little use for examinations in classics or chemistry. But the awarding of a scholarship at Oxford in classics or chemistry will always involve an essay to test the candidate's command of English and his capacity for thought. The examination for the scholarship will also call for a display of his mastery of English history and literature.

It would be a mistake to think of the preparation for winning a scholarship as a highly specialized one. Take scholars who have won their emolument in a subject like history. Some of them may elect to do part of the classics program after entering Oxford. They have been well enough prepared at school to keep up with the classics scholar in his special field. The scholarship examination is not designed to favor a narrow training.

This should be kept in mind when it is said that the English *university* student has a greater tendency to specialize than the American student. In a way this is true. He has a better basis for specialization, since he already knows a good deal when he comes to the university.

It should also be remembered that a lot of essay writing has taught him to apply what he knows. The thoroughness of his studies and the amount of independent or extra-classroom activity which is usually included give him a maturity in which the American fresh-

man is notably lacking. A great deal more attention has been paid by the school to developing his interests and finding out what he is best fitted to do. For all these reasons he is more capable of making an intelligent choice of his special field in the university. Interests are developed by hard work under the direction of a man who has every chance to see what the boy is like. This is a fact of which many American schools seem notably ignorant. We have a tendency to regard interest as the gift of a gracious divinity, so that not much hope is entertained of building it up by work. And since we often manage to put off hard work until after college, it frequently happens that our interests are not developed before middle age. Also our schools are overcrowded and have little opportunity for special attention to the promising boy, so that there is not much chance of noticing the germs of interest.

Boys who have been educated very intensively before they come to the university are prepared to do very astonishing things. It should be kept in mind that there are hundreds of these scholarships distributed among the twenty-one colleges at Oxford, and that many who do not hold scholarships were prepared to compete for them. The ordinary classics scholar coming to the university at eighteen or nineteen can run through the thousand odd lines of the Bacchae in a long evening. He reads scads of Demosthenes and Lucretius at sight.

Some educators and a large part of the American public would be repelled by this description of the scholarship system at Oxford because I have put the main emphasis on the extraordinary command of Latin and Greek which it produces. It would have been just as easy to dilate on the vigorous encouragement which is given by the scholarship system to modern history or science. But Oxford is chiefly interested in making sure that the best freshmen have a complete mastery of Greek and Latin. Oxford is even more strongly convinced now than it was a century ago that the highest type of university training, and the only type which does justice to men of real capacity, is an intensive study of the classics.

The reader who wishes to understand this policy must first dismiss his experience of the study of the classics in an American college or university. In the first place, the command of languages makes possible a kind of study which is almost unknown in this country, even among graduate students. A freshman who has gone to a good American prep school and has stood near the top of his class is ready to tackle an assignment of a couple of hundred lines of Ovid. To be told to read through the Metamorphoses in a couple of days or a week would stagger him. Any one of five or six hundred Oxonians could accomplish it. Thus they are in a position to treat the Metamorphoses as literature.

In the second place, the scholarship system has done its best to make sure that they are really saturated in English history and literature. That is something to be accomplished at home and in school, and in casual reading. Acquaintance with problems of the day is often stimulated at the dinner table. Politics is constantly in the foreground. Periodical material on these subjects is large in quantity and high in quality, and it is read. The classics scholar usually knows his own country far better than the student taking a course in "American national problems" knows his. The English undergraduate can afford to travel far afield because he is seeing a good deal at home.

The third reason for dissociating the classics program at Oxford from its American equivalent is that it succeeds in studying a civilization. In studying this civilization it puts its chief emphasis on philosophy. The American

teacher of Greek is nearly always anxious to point out to his students that he knows nothing about philosophy. Even when he is giving a course in the Platonic dialogues he makes a habit of referring to the philosophy department questions about the thought. This may be praiseworthy modesty on his part, but it is none the less a very unfortunate separation, and may lead to a serious misconception of Greek civilization or a serious loss of Greek contributions to present-day thinking. Even if one contended that a civilization could be understood without its philosophy, it would be hard to apply this rule to the Greeks.

The study of the Greek classics is the usual way of beginning philosophy at Oxford, and no one who is teaching philosophy in England has begun the subject in any other way.

American students of philosophy are not very completely saturated in any of the great literatures. This often makes their work barren, and gives it a technical flavor born of unreal problems. The work is remote from the main current of European reflection—Shakespeare, Goethe—and is correspondingly naive and small minded.

Plato was trained in a very different way, as is obvious to any reader of the Republic, with its frequent references to Homer and other poets. The English student is able to approach Plato with nearly the same background. He has been soaked in Plato's own literature. It is not uncommon for him to quote Homer in connection with a paragraph from the Republic, and he seems to do it with taste and aptness. At school and in his first two years at Oxford he becomes thoroughly familiar with Homer, Aeschylus, Sophocles, Lucretius, Vergil, Demosthenes, Cicero and some others from whom a selection can be made. Thus he begins his philosophical studies with a mind which differs very much from that of the usual student of philosophy in this country.

The program of Greats, or *literae humaniores,* which fills the Oxford undergraduate's last two years, has for its *pièces de résistance* Plato's Republic and Aristotle's Nichomachean Ethics. The first assaults are made on these in the long summer vacation, and they are read a number of times. In connection with each one of these books the student writes eight or ten essays. This is precisely half of all his work for a term of eight weeks. It should, therefore, be reckoned as equivalent to one half of our program, or two and one half courses, for eight weeks, or something more than a semester course devoted to each book. This leaves out of account the large amount of time devoted to each of them in the vacations. Normally the student has two essays each week, one in philosophy and one in history. For the latter subject he reads Thucydides and Herodotus, and modern works on Greek and Roman history. There is usually a term (eight essays, something more than a semester course in our terminology) for a book like Bradley's "Ethical Studies," and another term for logic, say again Bradley's. It should be remembered that before each term there comes a vacation (six weeks at Easter and Christmas). It is a time for soaking up what will later be formulated and criticized in the essays of the term itself. Both the logic and ethics studied with Bradley or Croce, and the term's work in modern philosophy from Descartes to Kant, will involve frequent rereadings of Plato and Aristotle, who have dealt perhaps more effectively with the same problems.

The men come back to English life with the framework of a civilization laid bare—and laid bare not by doctrinaire philosophy but by reflection springing out of a thorough saturation in all the relevant material, poetry, history, science. Science? Where does that come in? One of the most amusing delusions of the modern mind is its idea that science was born in the seventeenth

century. As a matter of fact it would not be hard to maintain that the Greek achievements were more important than let us say the English contribution. In order to understand the Athenian thinkers (Plato and Aristotle) who were saturated in Milesian and Sicilian science, the student of Greats becomes moderately familiar with the beginnings of science. While this naturally does not supply him with the knowledge of science which would be desirable in a perfect education, it gives him a far better chance to grasp the main purpose and method of science than is granted to the man who takes "baby physics" in one of our colleges. He also has a better chance to find a place for science in a unified scheme of human activities.

Dr. Meiklejohn in his experimental college at Wisconsin has been giving his freshmen a year with the Greeks. But this is to be sharply distinguished from the program I have just described, since the Wisconsin students have neither the mastery of Greek nor the wide acquaintance with American history, literature and current problems which make the Oxford venture a fruitful one. Nor do they spend a twentieth of the time which the English student gives to the mastering of a few monuments of the Greek genius.

This is a program which all the best students at Oxford are expected to follow, and they display considerable unanimity in electing it.

Many American students would resent being held down for several years to a minute study of Plato and Thucydides. Of course the man who has an interest in history and literature finds that Plato's remarks about artists and his criticism of institutions grow more fruitful the more he reads. When there is a background in the student's mind every return to the Republic means an expanding of the interest in history and literature. This well deserves to be called freedom, but it is not the license which the American student often imagines he will find at Oxford.

Very recently an attempt has been made to build up a modern equivalent for the concentrated survey of Greek and Roman civilization. It is called the school of philosophy, politics and economics. Modern English constitutional development, modern economic theory, the industrial revolution, the philosophy of Immanuel Kant—all these things give a special significance to the years after 1760. It was felt that if taken together they would provide a good view of the modern world. Unfortunately the philosophy tends to carry one back into the seventeenth century while the politics and economics move forward into the nineteenth. English philosophy in the nineteenth century is not nearly so fruitful or fascinating as that of the period which culminates in Hume (1750), and yet it is the natural accompaniment for the industrial revolution and parliamentary development. What is even worse, the literary background (which would make the program as solid and concrete as the equivalent study of the ancient world) is so enormous that it takes years to acquire it. The scholarship system which encourages saturation in English history and literature has done comparatively little to produce the same command of French and German masterpieces. The languages which would be essential are perhaps even worse taught than they are in the United States. There are scholarships in modern history and modern languages, but the holders of them have not usually read any modern philosophy at school, whereas the classics scholar has always read some Plato before he comes to Oxford. Thus in the modern field there is a separation of language, literature and philosophy in marked contrast to the union which is maintained between these elements for the student of the ancient world. Ordinary history is well taught at school, but the constitutional develop-

ment and the economic theory is passed over lightly, while as I have already pointed out there is no modern philosophy at school. Thus the whole preparation for this modern curriculum is far below what we find with the classics scholar.

The new program has been very popular with foreign students. Since it was started they have been able to work in philosophy without knowing Greek. Then there are always a number of Englishmen who have completed their four years of classics and now take a year to read some modern economics, brush up their history and really come to grips with Kant. It might almost be said that such a five-year program has become the rule with first-rate men. They also get a chance to polish up a modern language hastily learned at school, and to start on a new one. Considering the popularity of Italian philosophy and their command of Latin it is not surprising that this new language is usually Italian. The speed with which this modern curriculum is mastered by the classics student provides good testimony both as to his general training and his equipment in modern subjects.

There has been a good deal of talk in this country about the Oxford student's freedom from lectures. This is almost entirely a myth. The Oxonian attends about as many lectures as his American counterpart, or if he fails to go he is likely to hear about it at the end of the term when he is called up alone before the assembled dons of the college. This should be contrasted with the effort which has been made at Swarthmore to free honors students from lectures. Now of course many people at Swarthmore are perfectly aware that this is not Oxonian, and in any case Swarthmore is not pretending to imitate Oxford. But many Americans seem to believe that it is an Oxford idea. President Lowell is one of them. His reports often allege a contrast between Harvard adherence to lectures and Oxford neglect of them.

It should be remembered that the Oxonian is a good deal more highly developed intellectually than his American counterpart, and hence is capable of digesting a larger number of lectures in a given time. Moreover he rarely attends a lecture unless the book which is being commented upon in the lecture has been studied or at least read over with some care in the vacation. Usually he is devoting a good deal of time to the preparation of essays on the book at the same time that he is hearing lectures about it. He therefore goes to the lecture with a good many questions that he wants answered. At the same time he has the material organized to a point which makes it unnecessary for the lecturer to attempt to cover everything.

There is some freedom involved in the fact that no "homework" need be done for these classes. All the writing is done for tutors, of whom there are normally two (one for philosophy, the other for history), so that two essays, or occasionally three, must be turned out every week. That is all the work there is to do. The usual time for the preparation of an essay will vary from four to twenty hours. Thus some time is often left for reading which is more casual or bears less directly on the problem in hand. And the student has time to do the essay in any way which appeals to him. Only he must remember that a searching fire will be directed at irrelevance. If he wants to go far afield he must justify his point of view to the tutor. Anything he can bring to bear will be welcomed if he really does succeed in making it apply. There is elasticity here, where the mere assignment would prognosticate only a rehash of Plato. It is elasticity made possible by hard work in other fields and brought into connection with the Republic. And it is controlled by high standards as to what is and is not relevant.

Even the assignments themselves can be varied if the student will suggest a topic which fits into the general field the tutor has chosen for the work of the term. This subject could be changed only by shifting over to some honors school other than Greats (*e.g.*, English, history). Normally the tutor will run through a series of essay subjects, most of them old standbys. The relation of each term's work to the next is similarly guided by the Greats tradition, modified by the practice of the college and the tutor's preferences. The whole is related to an examination which will cover all the work.

Taking it by and large this is a mill which must be gone through, must be accepted for better or worse. This is not by any means a new thing for the undergraduate. In school he was early obliged to make certain choices in studies, and after that was put through a coherent routine. If he was picked out to be pointed for a scholarship he was released from some classwork and more attention paid to his special capacities, but the body of work to be done was considerably increased, while higher standards of coherence and relevancy were applied to its organization. He probably had very little to do with the making of the actual decisions as to what he would study, though efforts were made to consider what was likely to bring out his talents most effectively. In the same way he was probably put through a routine of games by all kinds of direct and indirect pressure. He was beaten by older schoolmates and masters if he broke any of the rules. There were a large number of conventions to which he had to conform. This is not altogether admirable, but it breeds a kind of ruthlessness about private impulses, a disregard for the merely comfortable in human relationships, which is quite exhilarating.

Every hour's discussion with the tutor presents a chance for elasticity in the treatment of prescribed subjects. But here too the student is not allowed to be formless. It is taken for granted that people who expect to talk must take some trouble to inform themselves about the subject. In the eyes of the tutor the Oxford student is often slack, but to the American teacher he would probably seem a marvel of intellectual curiosity. The Oxford tutorial is the only institution which approaches an editorial office in the severity of its criticism of bad writing, and it is just as hard on loose talking. English tutors visiting honors groups in this country often impress the American student as brutal. When they see an opening they swing from the canvas.

And the unreality of academic discussion is hateful to them. They give vigorous condemnation to the student's failure to drag his own opinion out into the open and give it an overhauling. He must not discuss issues which are remote for him. A good many sorts of academic pretense yield before the insistent effort to say what one really thinks, and then base the discussion on the issue so raised. This gives the talk a directness which is rude but refreshing. And those who emphasize the informality of the procedure and its closeness of personal contact are undoubtedly right in doing so. Only it is sometimes not noticed that the directness is strictly intellectual, that the informality is informality in brushing aside irrelevancies. The personal contact is a purely intellectual affair. The tutor's concern for private welfare and his friendliness in connection with the business of daily life disappear as soon as the grindstone of discussion has been brought forward. Behind this concentration on the problem lies a rigorous training for both tutor and student. Even the directness and informality of the tutorial are strictly conditioned by this training. It takes some practice to

put one's intellectual cards on the table, to put aside academic pretense. To the uneducated freshman who usually presents himself in our colleges it comes as rather a rude shock to be asked, "Do you really believe this stuff you've been quoting from the book I assigned you?"

This is very closely bound up with the brutal directness of Oxford college life. There is plenty of snobbishness, though it is not always in the ascendant at most colleges. Little effort is wasted trying to remain on friendly terms with those who do not meet with one's immediate approval, and no effort at all is made to conceal what one thinks of them. The play "Journey's End" represents a group of English public-school men in the trenches. The commanding officer has been drinking heavily and is approached on the morning after by the orderly.

"Like a plate of sardines, sir?"

"I should loathe it!"

It would be hard to find anything more typical of the English undergraduate's manner of expression. Americans have doubtless had the same feelings. They have doubtless refused the sardines, and even with harsh words. But there is something in the intonation and the idiom "I should loathe it!" of which few Americans would be capable.

In the last term before the comprehensive examination most of the meetings with the tutor are devoted to the answering of old examination papers. This puts a special emphasis on practice in bringing knowledge to bear on specific questions, and in this last period criticism of form and phrasing is brought into the foreground. There have been a good many examinations before the final test is reached, perhaps an average of one a term. But these serve simply as exercises in relevant writing, and as guides to future assignments. They are marked by the tutor concerned, who thus has another opportunity to bear down on looseness and unreality. They have absolutely no bearing on the degree or the class ranking secured from the university.

The comprehensive examination covering the whole of the work is an important feature of the routine, but it has received rather more than its due share of attention on this side of the Atlantic. Doubtless it is important to demand that the work shall not be crammed, and the comprehensive examination is a good way to make sure that this demand is met. But it is even more important to have a unified program which does away with cramming because it does away with fragmentary work. The elective system in this country has played havoc with unified programs; and it does not do much good to introduce comprehensive examinations until the elective system has been given up. It is not much use indulging in diatribes against cramming unless we root out the piecemeal idea of education which makes cramming practically inevitable. Piecemeal education is still in the ascendant, for instance, at Harvard. Swarthmore seems to have a clearer idea of what is meant by coherent work, but many of the programs are so crowded that the result is not much different from what one finds at Harvard. Neither at Swarthmore, Harvard nor at any other American institution of which I know has it done a great deal of good to introduce the comprehensive examination.

# ELECTRICAL EFFECTS REVEALED IN STARLIGHT

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ONE of the most fruitful and fascinating methods of modern astrophysical research is the application of new principles originating in pure physics to the study of the stars. It was in this manner that the father of astrophysics, Sir William Huggins, made his most notable discoveries. In the year 1859 Gustav Kirchhoff communicated to the Berlin Academy of Sciences a paper in which he demonstrated that a mass of glowing gas radiating light of certain definite colors, or wave-lengths, would absorb just these same wave-lengths, and no others, when placed between the spectroscope and a luminous body emitting a continuous spectrum of all colors. The emission lines were observed to give place to absorption lines, which appeared against the background of the continuous spectrum.

At the time when this new physical principle became known to the world, Huggins was already at work in his own little astronomical observatory at Tulse Hill, in the south of London. He immediately seized the opportunities offered by Kirchhoff's announcement and used the new knowledge for an interpretation of his spectroscopic observations. And "so it was with every new advance, in whatever field. Sir William read of it at once, discussed it with Lady Huggins, called it to the attention of his correspondents, and carefully examined into its bearings on astrophysics."[1]

In 1870 Huggins installed in his laboratory a large magnet for the purpose of studying the effects of magnetic force upon the spectral lines of various elements. In this he failed because his instruments did not give sufficient optical resolution, and it remained for Professor Zeeman, of Holland, to show (in 1896) that when a source of light, for example a radiating gas, lies within a magnetic field, its spectral lines are split up into a number of components. The complete theory of the Zeeman effect is one of the most difficult branches of modern physics, but a brief qualitative explanation may here be given.

According to the quantum theory light is produced by minute negatively charged particles, called electrons. Under normal conditions the electrons revolve in fixed orbits around a central particle, or nucleus, which is charged with positive electricity. The combination of a nucleus with one or more electrons revolving around it forms an atom of a chemical element, and may in many respects be likened to the solar system: the nucleus corresponds to the sun and the electrons take the places of the various planets in their orbits. An undisturbed atom, it is believed, neither produces nor absorbs light. But if a small, though definite, amount of energy is supplied to it, for instance in the form of heat waves, certain electrons jump from one orbit to another. Every time an electron jumps from a larger orbit to a smaller one, a certain small amount of monochromatic light, called a quantum, is emitted. A sufficient number of these quanta, when observed through the spectroscope, appear as a line of definite color.

Now, it is well known that the force exercised by a magnet must affect any material particles near it that are

[1] G. E. Hale, *Astrophysical Journal*, 37: 149, 1913.

charged with electricity. Consequently it seems perfectly natural that in the presence of a magnetic force the electrons will move around their nuclei in slightly "perturbed" orbits, that the size of their jumps will be altered and that the line originally appearing in the spectrum will be shifted to a different shade of color. The mathematical theory of perturbed electronic orbits predicts the appearance in the spectrum of several closely spaced lines in place of the original line. This group of components is the Zeeman pattern. Its character and degree of resolution depend upon the nature of the chemical element and upon the intensity of the magnetic force.

Zeeman's discovery proved to be of great importance to astrophysics. Nearly twelve years later Professor George Ellery Hale observed that certain absorption lines of the sun's spectrum, which usually appeared sharp and narrow, changed into a pattern identical with that of the Zeeman effect when the telescope was turned directly on a sunspot. This led him to conclude that sunspots are huge magnets and that the intensity of their magnetic force could be measured from the degree of resolution of the spectral lines.

The existence of magnetic effects in light naturally raised the question as to whether there might not be similar effects due to electric charges. In 1913 Professor J. Stark found that an electric charge placed close to the radiating atoms of certain luminous gases would change the spectral lines in structure and sometimes in position. The theoretical reason for this effect is somewhat similar to that for the Zeeman effect. The electrons are perturbed in their orbits under the direct influence of the electric charge, and the light-quanta are changed in wave-length, resulting, as before, in a color shift. Stark and other investigators have found that all chemical elements are subject to such electrical changes, but the resulting spectral patterns are not always the same. Certain elements, for example hydrogen and helium, are affected by comparatively small electrical charges, while other elements, for example iron, are affected only by very strong fields. Furthermore, it has been found that certain lines split up into a number of components, while others are merely shifted in wave-length. The electrical disturbances make themselves apparent in a large variety of ways, and considerable work remains to be done in the laboratory before we shall know all about them.

Even a weak electrical charge, such as can be collected on a rod of hard rubber by rubbing it with a piece of fur, could produce a change in the wave-length of light, but in actual practice rather strong sources of electricity are needed in order to produce measurable displacements in the spectral lines. Consider, for example, the hydrogen line beta, which is one of the strongest absorption lines in the blue region of the sun's spectrum. In a strong electric field this line splits up into a large number of polarized components, symmetrically spaced on both sides of the original position of the undisturbed line. Two of these components are particularly strong and these alone appear in a spectroscope of small resolving power (Fig. 1). If the field corresponds to one unit on the centimeter-gram-second-electrostatic system, the wave-lengths of the outermost components will differ from the wave-length of the original line by about six one hundredths of an Ångström unit. On a photograph obtained with the spectrograph of the Yerkes Observatory this would amount to a displacement of about six one thousandths of a millimeter. In order to understand the amount of electricity in one unit on the centimeter-gram-second-electrostatic system imagine two metallic balls, each one gram in weight, one of which is station-


FIG. 1. THREE TYPICAL STARK PATTERNS

AS SEEN IN A SPECTROSCOPE OF MODERATE RESOLVING POWER. THE NORMAL UNDISTURBED LINE IS SHOWN AT THE TOP, AND THE PATTERNS BENEATH THEM. (A) HYDROGEN-BETA LINE; (B) HYDROGEN-GAMMA LINE; (C) HELIUM LINE 4472, SHOWING "FORBIDDEN" COMPONENT AT WAVE-LENGTH 4470.

ary while the other is suspended on a string about one meter in length. If both balls are charged with the same amount of positive electricity the stationary ball will repel the suspended ball. The greater the charge of electricity the stronger will be the repulsion. If the charge is such that the suspended ball is repelled to a distance of one centimeter from the stationary ball, the quantity of electricity in each ball is said to be one unit on the above-mentioned system. One such unit working at a distance of one centimeter would produce a splitting of the hydrogen line corresponding to a displacement of about six one hundredths of an Ångström unit, in both directions from the center. For stronger electric charges the displacements become greater, and as an approximation it is usually sufficient to assume that the line-displacement is proportional to the intensity of the electric field. A doubling of the charge produces a doubling of the line-shift.

In practice we do not always use the unit of electricity defined above. The field intensity is often expressed in volts per centimeter. One volt per centimeter corresponds to one three hundredth part of the electrostatic unit, and the conversion from one system to another is performed by simply dividing or multiplying by 300. For example, in the case of the hydrogen beta line a field of 100,000 volts per centimeter will shift the outermost component to a distance of approximately twenty Ångström units from the original wave-length.

If a star were a charged sphere, comparable to a charged ball of metal in the laboratory, it should produce a Stark pattern in the absorption lines of its spectrum. Astronomers have looked for such patterns but have found none. The most extensive and by far the most accurate investigation of this nature was undertaken some fourteen years ago by Dr. Hale and Dr. Babcock at the Mount Wilson Observatory. They concluded that the electric field strength at the level where the lines of the sun's spectrum originate is less than 200 volts per centimeter. Accordingly, if the gaseous sphere constituting the sun's bulk is charged at all, it can contain only a comparatively small amount of electricity. The same is true of all stars thus far investigated.

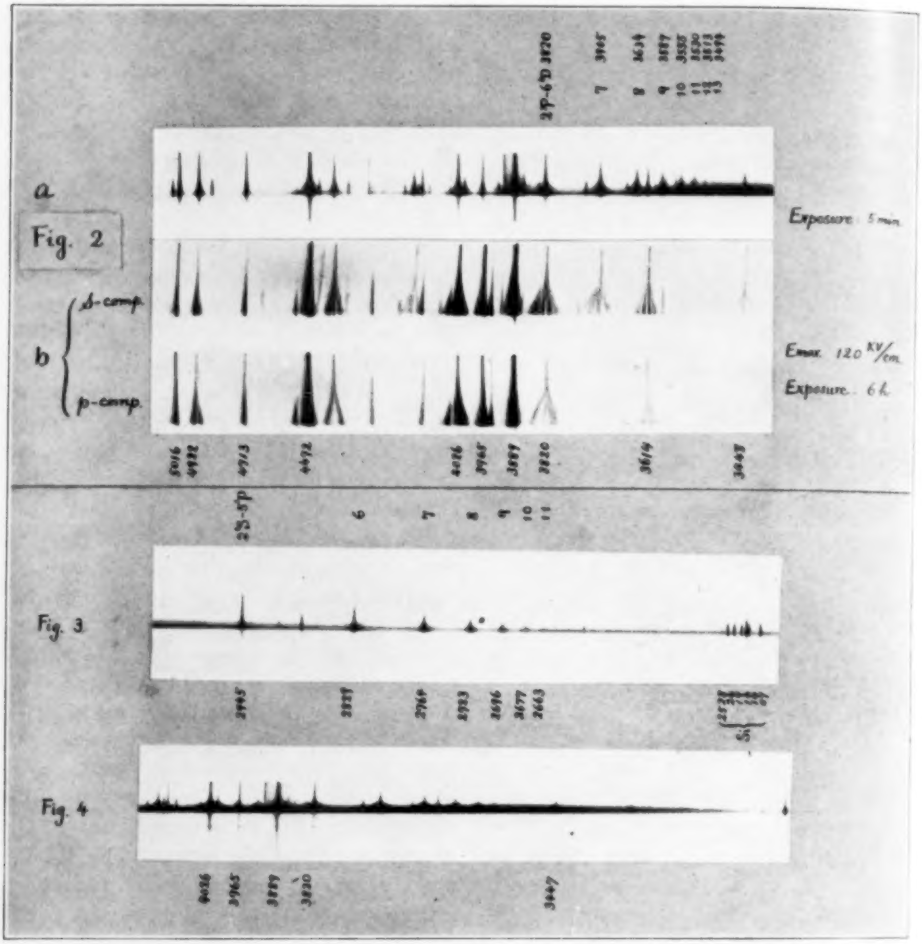
The absence of a real Stark pattern in the spectral lines of a star does not necessarily mean that no effects of electrical forces are present. The star may not be like a charged ball of metal brought into close contact with a source of light, perturbing the electronic jumps. Laboratory experiments have shown that electrical effects may be observed in luminous gases in which there are no visible sources of electric force. As a matter of fact, a gas itself contains a sufficient amount of electricity to produce perturbations in the electronic jumps. Under ordinary conditions the positive charge of the nucleus of any given atom is firmly bound to one or more electrons, the whole forming a neutral atom. However, if a gas is "energized," as when it is heated to a high temperature or exposed to ultraviolet radiation, some of its atoms may

lose one or more electrons. The process involved is called ionization.

Under the influence of energy coming from the outside certain atoms in the gas may be forced to throw off one or more of their electrons. These electrons dash off with velocities depending upon the amount of energy swallowed by the atom. If an atom swallows a very small amount of energy, the electron may not be loosened sufficiently from its bonds to escape completely. The atom becomes temporarily excited, but it soon returns to its original state and throws out its excess energy in form of light. If the amount of energy swallowed by the atom is just sufficient to loosen the electron, the latter will leave the atom, but will not be supplied with energy in the form of motion. It will remain stationary and will finally be caught by some other atom that may also have lost one electron in the past and will thus be eager to capture any stray electrons in its path in order to complete its electronic family. If the energy from the outside is very great, the excess over what is needed to loosen the electron from the parent atom is bestowed upon the electron in the form of motion. Such an electron will move with great speed through the gas until it happens to hit another atom and becomes a captive once more.

In the atmospheres of the stars the gases are known to be highly ionized. From a study of their spectral lines we find that most atoms have lost at least one electron and many have lost two, three or even more of them. All these free electrons are moving about in the gas. Each is charged with a minute quantity of negative electricity, equal to 0.0000000005 electrostatic unit. As we have already seen, an ordinary atom is electrically neutral. In order to be so, it must contain an amount of positive electricity just equal to the sum of all the negative charges collected on the electrons. As soon as one electron is knocked off the atom, the remaining part, the ion, has an excess of positive electricity left over, and consequently there will be in the gas not only negative charges moving about, but also an equal quantity of positive charges collected on the ions.

Now suppose that in its flight through the atmosphere of a star such a charged particle—ion or electron—happens to pass very near to a hydrogen atom that is about to radiate the hydrogen beta line. What will happen? Naturally the electric charge will cause perturbations in the electronic orbits of the radiating atom and the emitted line should display the characteristic features of the Stark effect. It is true that the charge of a single electron is extremely small, so small, indeed, that it may not at once be clear how it could possibly produce a measurable resolution of the spectral line. But the disadvantage of possessing so small a charge is offset by the fact that an electron can approach a radiating atom much more closely than could a visible source of electricity. We have seen before that a charge equal to one unit on the electrostatic system, placed at a distance of one centimeter from the radiating gas, will displace the outermost components of the hydrogen beta line by an amount equal to about 0.06 of an Ångström unit. This is a small displacement; we should consider a charge at least ten times as large, say ten units, in order to produce a resolution that would be readily discernible. The charge of an electron is 20,000,000,000 times smaller than this quantity. The question is, of course, how closely the electron approaches the radiating atom. At a distance of one centimeter the resolution would be practically zero; but in a gas there are really no limitations for the approach between electron and atom other than their respective sizes. The size of the electron



—*From the Astrophysical Journal.*

FIGS. 2 TO 4. THE STARK EFFECT IN HELIUM

AS PHOTOGRAPHED IN THE LABORATORY BY DR. TARO SUGA AND DR. Y. FUJIOKA, OF TOKYO, JAPAN. FIG. 2 REPRESENTS THE STARK EFFECT IN THE BLUE AND VIOLET REGIONS; (a) SHOWS THE MOLECULAR ELECTRIC BROADENING OF THE LINES, DUE TO THE PRESENCE OF CHARGED ELECTRONS AND IONS. THE LABORATORY CONDITIONS WERE MADE TO REPRESENT AS NEARLY AS POSSIBLE THE CONDITIONS INSIDE THE ATMOSPHERE OF A VERY HOT STAR; (b) SHOWS THE STARK EFFECT PRODUCED BY A CONSTANT ELECTRIC FIELD. MANY LINES ARE SPLIT UP INTO COMPONENTS. FIGS. 3 AND 4 SHOW THE STARK BROADENING OF SPECTRAL LINES IN THE ULTRA-VIOLET PART OF THE SPECTRUM.

is so small that it can be neglected; that of an atom varies according to the species considered, but in general it may be assumed to be not larger than 0.00000001 centimeter. Consequently an electron can approach an atom to a distance of one half this amount, or 0.000000005 centimeter. The electric force which, when exerted upon the atom by the electron, causes the perturbations in the electronic jumps, varies inversely as the square of the distance. While the

charge is 20,000,000,000 times less than that of our visible charge of ten units, the total value of the force would prove to be much greater; it would equal $\frac{(200{,}000{,}000)^2}{20{,}000{,}000{,}000}$, or approximately 2,000,000. The resolution produced by an electron at its minimum possible distance turns out to be two million times greater than that produced by a visible charge of ten units at a distance of one centimeter.

However, the motions of atoms, ions and electrons in a gas are distributed at random. Consequently only a very small proportion of the free charged particles will approach the radiating atoms to the nearest possible distance of 0.000000005 centimeter. The majority will fly past the atoms at somewhat greater distances, depending chiefly upon the degree of pressure applied to the gas. In a compressed gas the particles—atoms, electrons and ions—are so closely packed together that the average distances between them are small. On the other hand, if the gas pressure is low, the distances are correspondingly larger, and the observed effects upon spectral lines should be smaller. In the case of a stellar atmosphere the average pressure may be estimated from the work of Russell,

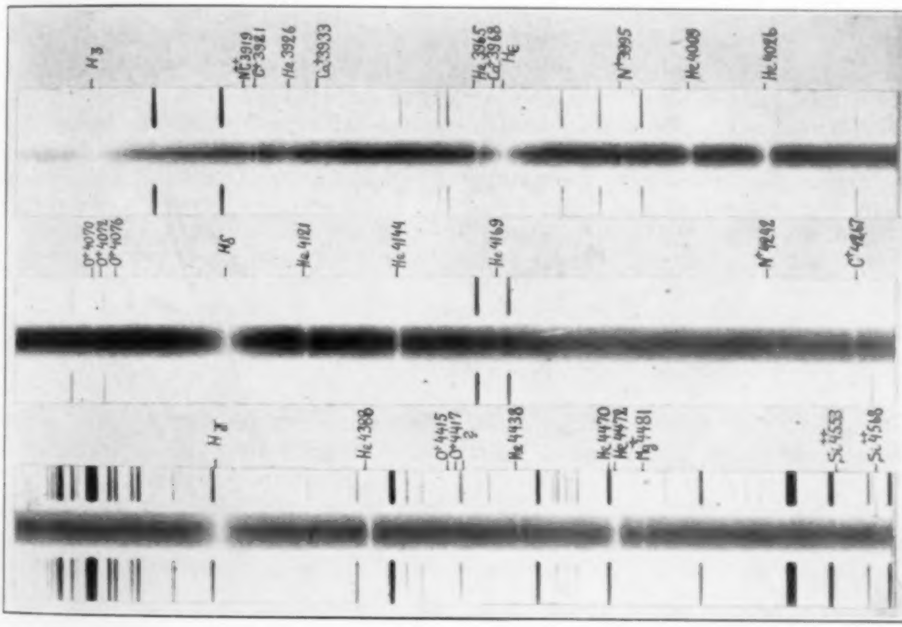



**FIG. 5. SPECTRUM OF THE STAR 88 GAMMA PEGASI**

AS PHOTOGRAPHED WITH THE BRUCE SPECTROGRAPH OF THE YERKES OBSERVATORY ON JULY 17, 1929, AT $7^h08^m$ UNIVERSAL TIME. THE THREE STRIPS REPRESENT VARIOUS PORTIONS OF THE SPECTRUM. THE BLACK BAND IN THE MIDDLE OF EACH IS THE CONTINUOUS SPECTRUM OF THE STAR. THE STELLAR ABSORPTION LINES (WHITE IN THE PHOTOGRAPH) CROSS THE CONTINUOUS SPECTRUM. THE BLACK LINES AT THE TOP AND BOTTOM OF EACH STRIP ARE DUE TO THE TERRESTRIAL COMPARISON SPECTRA OF IRON AND TITANIUM. NOTICE THE GREAT WIDTH OF THE HYDROGEN LINES Hγ, Hδ, Hε AND H ζ. NOTICE ALSO THE GREAT WIDTHS OF THE HELIUM LINES 3926, 4009 AND 4026 AS CONTRASTED WITH THE SHARPNESS OF THE HELIUM LINE 3965. THIS IS IN AGREEMENT WITH THE REQUIREMENTS OF THE STARK EFFECT. THE LINES OF THE HEAVIER ELEMENTS ARE ALL SHARP AND NARROW, *e.g.*, Mg 4481, Si 4553, ETC.

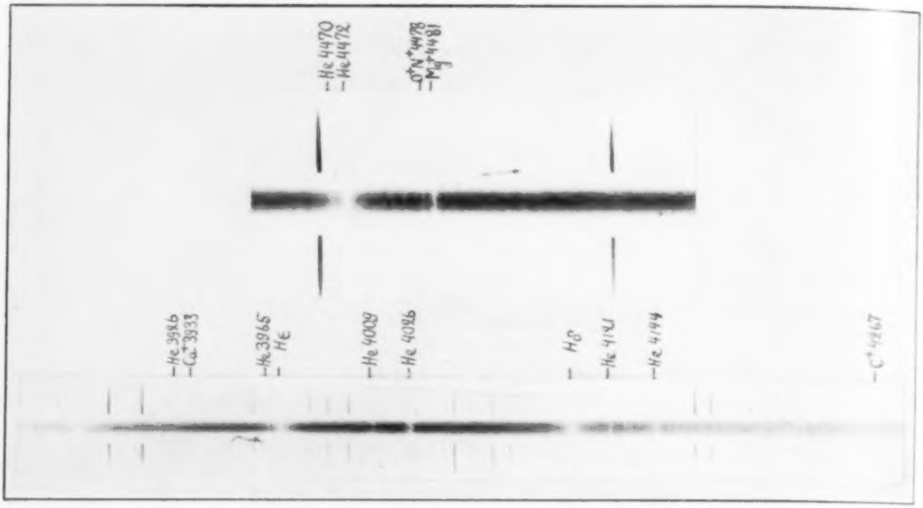



FIG. 6. SPECTROGRAMS OF TWO STARS

AT THE TOP IS A HIGH-DISPERSION SPECTROGRAM OF THE STAR 88 GAMMA PEGASI, OBTAINED AT THE YERKES OBSERVATORY ON SEPTEMBER 21, 1929, AT $4^h 38^m$ UNIVERSAL TIME. THE "FORBIDDEN" HELIUM LINE AT WAVE-LENGTH 4470 IS SHOWN TO THE LEFT OF THE STRONG PERMITTED HELIUM LINE 4472. NOTICE ALSO THAT THE TWO HELIUM LINES—PERMITTED AND FORBIDDEN—ARE APPRECIABLY BROADER THAN THE MAGNESIUM LINE 4481 OR THE LINE AT 4478 WHICH IS A BLEND OF IONIZED OXYGEN AND IONIZED NITROGEN.* THE "FORBIDDEN" LINE CAN NOT BE PRODUCED IN THE HELIUM SPECTRUM EXCEPT IN THE PRESENCE OF ELECTRIC FIELDS. THE WIDENING OF THE HELIUM LINES IS IN AGREEMENT WITH THE STARK EFFECT. AT THE BOTTOM IS A SPECTROGRAM OF THE STAR 85 IOTA HERCULIS PHOTOGRAPHED ON JULY 3, 1929, AT $7^h 11^m$ UNIVERSAL TIME. THE HYDROGEN LINES ARE VERY WIDE, DUE TO STARK BROADENING.

* Since the above was written, this line has been identified by the writer as due to doubly ionized aluminum, wave-length 4479.89.

Milne and others. Professor Russell and Professor Stewart, of Princeton, have computed the average distance to which charged particles will approach a radiating atom in a stellar atmosphere and have found that the effect upon spectral lines, such as those of hydrogen, should be well within the accuracy of measurement.

Let us now consider the observational side of the matter. If we were dealing with one single radiating atom and one charged particle, the observed result would be a splitting up of the spectral line into the ordinary Stark pattern. But in reality we are not observing a single atom. The light coming to us from a star is the integrated effect of an enormous number of separate atomic acts. A moderately strong line in a stellar spectrum is produced by the combined action of something like $10^{42}$ absorbing atoms. Every individual atom is subject to perturbations produced by the nearest charged particles. But while in some cases these particles may happen to be very near, producing a wide resolution of the line, other atoms may be rather far away from any electrons or ions and will give rise to narrow Stark patterns. The combined effect is, then, a composite picture due to the superposition of $10^{42}$ individual Stark patterns of all possible resolutions, resulting in a broad and hazy line in which the individual patterns can not be recognized.

Assuming our conception of the star's atmosphere with its free electrons and charged ions to be correct, we should expect to find broad and ill-defined absorption lines for such elements as are known to be susceptible to Stark effect. This is what is actually observed. The hydrogen lines are always broad. In many stars they reach a width of 100 Ångström units, sometimes even more. Somewhat lesser widths have been recorded for the helium lines. Both elements are known to be susceptible to Stark effect and their widening in stellar spectra is in excellent accord with our expectations.

Until recently, however, there was no proof that the observed broadening was not produced by some process other than Stark effect. To interpret the vast mass of facts that a photograph of a star's spectrum presents is by no means simple. Broad lines can be due to a great variety of causes, and some of these were actually known to be present. It was therefore necessary to eliminate all known factors affecting the width of a spectral line and to find whether there remained a broadening that could be attributed definitely to Stark effect. The molecular Stark effect demands very definite forms for every given line, and it was necessary to determine whether the behavior of the stellar lines was actually in conformity with these theoretical requirements.

The most essential of these demands are the following.

(1) The amount of broadening caused by molecular Stark effect should be roughly proportional to the degree of resolution observed in the laboratory. A line that shows a large separation in a weak field should appear more broadened than a line that is less susceptible to electrical perturbations.

(2) In many cases the ordinary Stark effect is not symmetrical. In fact, the perfectly symmetrical pattern is characteristic only for hydrogen. All other elements show more or less asymmetry in their Stark resolution. Corresponding to this asymmetry we should expect an unsymmetrical broadening of certain lines in the stars.

(3) Under the stimulus of the electric field new spectral lines make their appearance. Such lines are "forbidden" under normal conditions, and their appearance in any spectrum is a good indication of the presence of electric fields.

(4) The Stark effect is most pronounced for elements of low atomic number. The heavier elements are only slightly susceptible to electrical perturbations. Hydrogen shows the greatest resolution; helium follows next; then come the other light elements. In the stars, then, we should expect, primarily, hydrogen and helium, and possibly nitrogen and oxygen, to exhibit the broadening effect. The other light elements are either absent in the stars or show such weak lines that no accurate measures can be made of them.

(5) The lines of any given element are not equally affected by electric fields. In helium, for example, there are lines that are almost as susceptible to Stark effect as the hydrogen lines, while others remain almost unaffected, even in the presence of very strong fields.

(6) Within any given spectral series (a group of related lines in the spectrum of a single element) the Stark effect is weakest for the lines situated farthest toward the red end of the spectrum and increases toward the violet and ultraviolet.

These several characteristics were tested[2] for the helium and hydrogen lines in the spectra of many stars. All were found to be present. Those helium and hydrogen lines which are subject to Stark broadening invariably appear

[2] *Astrophysical Journal*, 69: 173, 1929; 70: 85, 1929.

fuzzy and wide, even though the heavier elements, such as magnesium or silicon, show perfectly sharp and narrow lines. The observed broadening increases toward the violet as we proceed along any given series. Many helium lines are observed to be unsymmetrically broadened—in excellent agreement with the laboratory patterns of the Stark effect. But of greater significance than any of these is the identification of the "forbidden" helium lines with a number of previously unidentified stellar absorption lines. Thus we find the evidence overwhelmingly in favor of the existence of molecular electric fields in stellar atmospheres.

An advance of scientific knowledge in one direction frequently leads to new developments in other directions. A study of the hydrogen and helium lines in many stars disclosed the fact that the intensity of the Stark effect is not the same for all stars. As we have seen, the resolution of the lines in the individual Stark patterns depends upon the distance between atom and charged particle, and this distance, in turn, depends upon the average gas pressure in the stellar atmosphere. Unless this is the same in all stars the average distance between atom and charged particle will not be the same. Hence the observed broadening of the line would serve as an index of the pressure. Such a relationship has been derived mathematically and tested by observation; by means of accurate measurements of stellar spectrograms the exact value of the pressure, and hence of the density, may be determined.

The atmospheric density is a very important characteristic of every star; it is closely related to the volume. A so-called "giant" star has a large volume, and consequently the gravitational attraction at its surface is small. Such a star has an atmosphere of low density and shows little, if any, Stark effect. On the other hand, a "dwarf," though nearly as massive as a giant, has all its mass concentrated into little space, and the gravitational pull at its surface is strong; it has a dense atmosphere and displays a strong Stark effect.

Much remains to be done before we shall be able to form a complete picture of the molecular electric phenomena in the spectra of the stars. Possibly certain lines of nitrogen and oxygen may also be found useful in determining the intensity of the Stark effect. But the laboratory data relating to these elements are still incomplete, and their astrophysical interpretation is therefore difficult.

So far as we can tell at present, the average molecular field in a stellar atmosphere is of the order of 1,000 to 10,000 volts per centimeter. Such a field would not be considered strong from the laboratory point of view, where fields of several hundred thousand volts per centimeter are produced. The average pressure would be something like 10,000 times less than that of air at the surface of the earth, a condition resembling, in the ordinary sense, a "vacuum" rather than an atmosphere.

# THE PROGRESS OF SCIENCE

## PROFESSOR DEMPSTER AND THE AMERICAN ASSOCIATION PRIZE

THE seventh award of the American Association prize of $1,000 was made at Des Moines to Dr. A. J. Dempster, professor of physics in the University of Chicago. This prize is awarded annually to the author of a notable contribution to the advancement of science presented at the annual meeting. The funds for the prize are generously supplied by a member who does not wish his name made public. The committee on award was this year composed of the following members: Charles E. Allen, University of Wisconsin, *chairman;* P. W. Bridgman, Harvard University; Fay-Cooper Cole, University of Chicago; S. C. Lind, University of Minnesota; H. L. Rietz, University of Iowa.

Dr. Dempster's contribution constitutes an important extension of the work of the French physicist, Louis de Broglie, for which the Nobel prize in physics was recently awarded. Dr. Arthur H. Compton has said that "the most important contribution of twentieth century physics is that the physical world can be reduced to three kinds of particles, protons, electrons and photons, and that each of these particles has also the characteristics of waves. The last stage of this work is the proof that protons, the positively charged parts of matter, have wave characteristics. It is this completion of the great work of twentieth century physics which has been accomplished by Professor Dempster."

According to Professor Henry G.

PROFESSOR A. J. DEMPSTER AT WORK IN HIS LABORATORY

Gale, president of the American Physical Society, "physicists, seeking to explain the ultimate nature of things, had believed that light was simply a wave form, and that atoms, which consist of negative electrons revolving around a positive nucleus, or proton, were simply particles. Professor Compton in 1926 proved that light is not only a wave but also a stream of particles, or photons, bundles of radiant energy. In 1927 two investigators at the Bell Laboratories, Davisson and Germer, revealed that the negative electrons are not only particles but that each particle also acts as a wave. The final evidence in the cycle has now been adduced by Dr. Dempster, who has proved that the positive protons have also wave-form as well as particle-form. These three discoveries are probably the most striking advances in physics in recent years."

Professor Dempster, during the last eight months of experimentation, succeeded in taking photographs of the positively charged hydrogen nuclei in a manner which proved them to be vibrating approximately 1,000,000 times as fast as ordinary light. By shooting the protons through a crystal in a vacuum, he was able to show pattern marks on the sensitized plate. If the proton had been merely a particle without vibration, nothing but a dot would have been revealed on the photographic plate.

This "diffraction pattern" was explained by Professor Dempster as being similar to the diffused appearance of sunlight through an umbrella, a phenomenon peculiar to things having a wave-form. "In these experiments a calcite crystal takes the place of the umbrella, and is used as a mesh to control the particles. The hydrogen protons in these experiments penetrated the crystal in much the same way that light photons would, and their patterns on the photographic plate leave no doubt as to their wave-properties. The experiments completed have so far been confined to hydrogen. But since all the elements consist of the same thing or things, in different arrangement, it can be assumed that the protons of all substances would act in the same manner. Hydrogen is the simplest of the elements. We are now applying the method to helium, the next simplest. How protons, electrons and photons can be both waves and particles at the same time is perhaps the greatest problem now confronting physicists. This experiment does not throw direct light upon that obscure relationship but helps to clarify the problem."

Dr. Dempster, who is forty-three years old, has been a member of the physics department of the University of Chicago for fifteen years. He is a graduate of the University of Toronto and of the University of Chicago, and has studied at Göttingen, Munich and Wurzburg.

## THE TOTAL ECLIPSE OF THE SUN IN CALIFORNIA

The first total eclipse of the sun for 1930 will occur on April 28 across a narrow strip of California about half a mile wide. In answer to a request by the University of California that the army air service cooperate with Lick Observatory in observations of this eclipse, President W. W. Campbell has received word from the U. S. army headquarters in San Francisco that the requisite type of plane is available and that such cooperation will be gladly offered.

Tentative plans call for the services of one plane at an altitude of not less than 10,000 feet, over Napa Valley, between Napa and St. Helena, and perhaps of

Christine Ladd-Franklin

LECTURER ON PSYCHOLOGY AND LOGIC AT COLUMBIA UNIVERSITY, DISTINGUISHED FOR HER CONTRIBUTIONS TO THESE SUBJECTS, WHO DIED IN NEW YORK ON MARCH 5, AGED EIGHTY-TWO YEARS.

another plane equipped to take moving pictures of the eclipse shadow and of the eclipse itself.

In explanation of the cooperative plan, President Campbell says:

The purpose of the airplane plan is to carry the observer well above the highest clouds which might be in the way of eclipse observers located on the earth's surface. The total phase of the solar eclipse will last not more than one or two seconds. The shadow of the moon on the earth's surface will be not more than one half or five eighths of a mile in width, and for an observer to locate his observing station where the shadow of the moon in its rapid northeastern travel will pass directly over him and give him the second or two of totality will be difficult, and it is quite likely that the slight uncertainties in our knowledge of the precise position of the moon will leave many intending observers a little too far north or a little too far south to have the shadow pass over them.

The sky will not be very dark at the instant of totality because the moon will succeed in just a little more than covering the sun's image. I should guess that the observers who are fortunate enough to find themselves for a second or two in the shadow of the moon could read fine newspaper print about as easily in the open air as they would a minute or two before sunset on a clear day. This estimate, however, is very uncertain, but there will be no difficulty whatever in reading ordinary newspaper print at the time and place of totality.

The university is now preparing an observing station to photograph the eclipse from the earth's surface near Camptonville, Yuba County. This expedition, which will be under the charge of Dr. J. H. Moore and Dr. D. H. Menzel, is financed by William H. Crocker, of San Francisco, chairman of the regents of the university.

The plane to be used for high altitude observations will be of the open-cockpit type, of such construction that the upper wing will not interfere with the view of the observer or of the pilot. For a few moments before and after the eclipse, the plane will fly in the direction in which the shadow moves, northeastward, in order to increase by a little period the length of time that the total eclipse may be viewed.

It is suggested that there might be some interest in the taking of moving pictures of the eclipse, as also of the shadow on the ground, providing that it has edges sharp enough.

Concerning the accuracy of the predictions of the path of the eclipse, Professor Ernest W. Brown writes in *Science:*

The brief duration and the narrow path of totality of this eclipse, visible in California, make the question of the accuracy of the predictions of some interest to observers. Perhaps a brief statement of the problem, free from technical terms, may be of value.

The errors of the prediction arise mainly from two sources, namely, those due to lack of full knowledge of the motions of the earth, moon and sun and those due to the topography of the moon's limb. Owing to a variety of causes, the former have been much diminished during the past six years. There is still some uncertainty due to the changing rate of rotation of the earth, but this affects the position of the path less than it does the time at which totality occurs. In the coming eclipse the position is of importance because a small error may cause the observer to miss totality altogether. In a discussion just published in No. 934 of the *Astronomical Journal*, Dr. D. Brouwer has used all the latest available information, and we estimate that the uncertainty, due to these causes, of the position of the path given by him in a direction perpendicular to the path is less than a quarter of a mile.

The uncertainty due to the topography of the moon's limb is larger than this. We have plenty of evidence of valleys on the limb a mile or more deep and of mountains even higher than this amount, but our knowledge of their exact position at the time of the eclipse is not sufficient to enable us to use it for prediction. A valley a mile deep at either of the positions where the grazing edges of the sun and moon give the limits of totality on the earth may alter the edge of the path on the earth by a like amount. In the present case, where the predicted width of the path is considerably less than a mile, it may result in a complete absence of totality anywhere. Professional observers are aware of these facts, and have made up their programs accordingly. Certain classes of observation can be usefully undertaken with these conditions in view.

PAUL ADIN LEWIS

FORMERLY ASSOCIATE MEMBER IN THE DEPARTMENT OF ANIMAL PATHOLOGY AT THE ROCKEFELLER INSTITUTE FOR MEDICAL RESEARCH, IN WHOSE DEATH AMERICAN MEDICINE LOSES A LEADER. DR. LEWIS DIED OF YELLOW FEVER IN BAHIA, BRAZIL, WHILE INVESTIGATING THE CAUSE OF THE DISEASE.


*—Photograph from the New York State Museum*

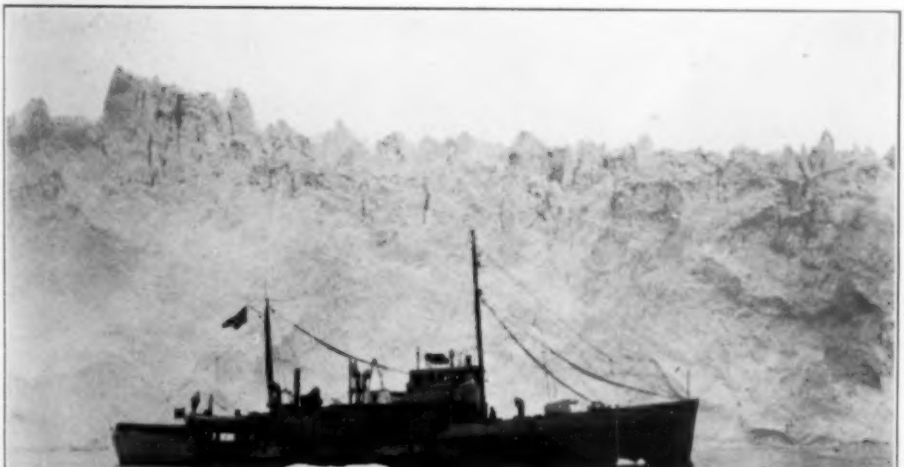
THE JOSEPH HENRY MONUMENT

RECENTLY UNVEILED IN ALBANY. IN THE BACKGROUND IS THE ALBANY ACADEMY WHERE JOSEPH HENRY MADE HIS IMPORTANT DISCOVERIES IN ELECTROMAGNETISM. IN 1846 HE WAS ELECTED AS THE FIRST SECRETARY AND DIRECTOR OF THE SMITHSONIAN INSTITUTION IN WASHINGTON, AND ITS ORGANIZATION WAS LARGELY DUE TO HIM.

## THE MARION OCEANOGRAPHIC EXPEDITION

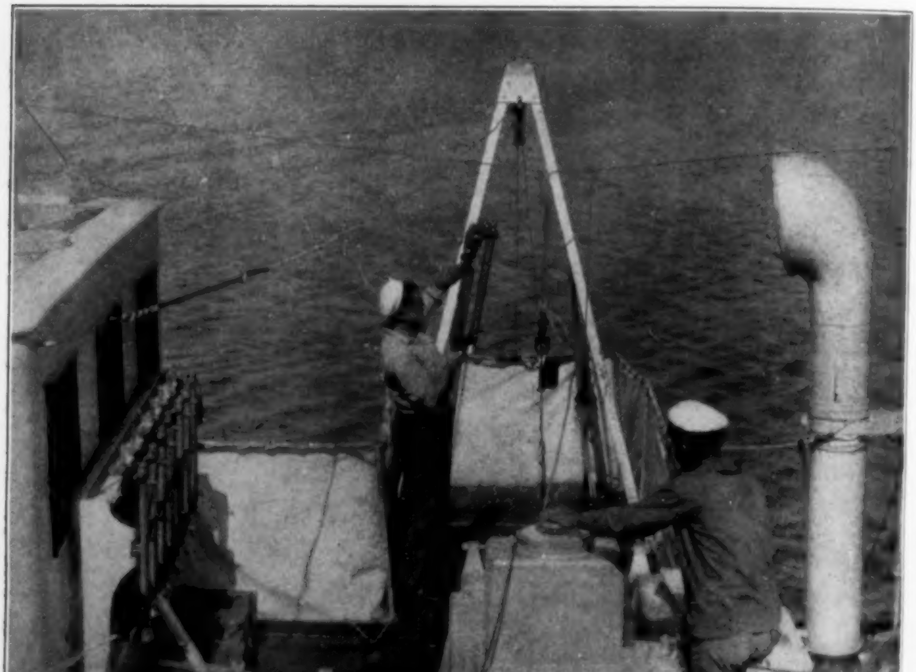
From information given to us by the U. S. Coast Guard we learn of the *Marion* Expedition. Under the command of Lieutenant Commander Edward H. Smith, it sailed into the far north to investigate the currents, ice, weather and other conditions in the furtherance of a knowledge of oceanographic and physical conditions of that vast, sparsely explored water area between the North American continent and Greenland. The expedition cruised 8,100 miles, covering with an oceanographic survey a 450,000 square mile area. A total of 190 observation stations were occupied at carefully selected positions in these waters, and about 1,900 observations of the temperature and salinity of the water were made. The temperature and water samples were taken at various levels from the surface down to near the bottom, use being made of Negretti and Zambra reversing thermometers and Greene-Bigelow water bottles clamped on steel wire cables with over three miles of the wire suspended from the ship. A special bottom-sampling tool was used at many of the stations to obtain good-sized samples of the muds and oozes from the ocean floor. In all 2,000 salinity tests were made on board with the salinometers. These instruments, the only ones of their kind, determine the salinity by means of measuring electrical conductivity of sea water after it has been placed in a special glass cell and brought to a very carefully regulated temperature of 25° Centigrade by a water bath. This instrument was developed by Dr. F. Wenner, of the Bureau of Standards, Washington, D. C., in response to an appeal of the International Ice Patrol service for a quick and accurate method of determining the salinity of sea water and one adaptable to the arduous conditions met on shipboard. Lieutenant Commander Edward H. Smith has been identified with the Ice Patrol service since 1920; from 1922 to 1927 he was the oceanographer of the Ice Patrol and is an authority on icebergs and ice problems of the North Atlantic.

Backed by the indorsement of Coast



THE CUTTER *MARION* AT THE BASE OF THE GLACIER AT PORT DE QUERVAIN

Guard officials, Commander Smith has in the past few years adapted the so-called Bjerknes formulas of free motion to tracing movements of the dangerous icebergs that every spring infest the waters off the Grand Banks. This work under the direction of the U. S. Coast Guard does much to insure the safety of the transatlantic liners and eliminates the probabilities of another *Titanic* disaster. Few of the general public realize that the ocean areas off Newfoundland are being mapped as to currents every two weeks during the Ice Patrol season and that the various tongues of the Labrador current and the Gulf Stream swirling menacing icebergs into the paths of commerce are plotted like the isobars on a weather map by the Weather Bureau. The Ice Patrol ships, *Tampa* and *Modoc*, each night and morning issue by radio to all approaching ships their ice warnings and forecasts. The current and ice maps constructed immediately on board the ice ships for dissemination to North Atlantic navigators represent a great practical application of oceanography and



—*U. S. Coast Guard*

MAKING OCEANOGRAPHIC MEASUREMENTS ON THE INTERNATIONAL ICE PATROL

THE MAN ON THE LEFT IS LOOKING TO SEE IF THE REVERSING THERMOMETERS IN THE GREENE-BIGELOW WATER BOTTLES ARE PROPERLY ADJUSTED. HE WILL NEXT CLAMP THE BOTTLE TO THE SOUNDING WIRE WHERE IT HANGS OVER THE SIDE FROM THE SHEAR LEGS AND SIGNAL TO THE MAN AT THE WINCH TO LOWER AWAY. THE WIRE WILL BE STOPPED WHEN THE MOTOR WHEEL SHOWS THE BOTTLE HAS REACHED THE PROPER LEVEL, WHERE SAMPLES ARE TAKEN OF THE SEA WATER FOR ANALYSIS.

—U. S. Coast Guard

NATIVE GREENLANDERS AT GODTHAAB, GREENLAND

are a fine tribute to the U. S. government administration of this important international safety work.

Since the famous *Challenger* Expedition, undertaken by the British from 1872 to 1876, many governments and quite a few private individuals and institutions have carried out scientific oceanographic investigations. The U. S. Coast and Geodetic Survey and the U. S. navy have both been identified with notable work along oceanographic lines. A famous pioneer in the physical geography of the oceans was an American naval officer, Lieutenant Matthew Fontaine Maury, and so in sending out the *Marion* our government has in a sense embarked again on a field of exploration in which we have a fine heritage. Practically no oceanographic

ROPING A POLAR BEAR FOR THE NATIONAL ZOO IN WASHINGTON

research work by any one has as yet been done in the area covered by the *Marion*.

It is a little early to draw many final conclusions from the data obtained. However, such great progress has been made with the working up of the observations on account of the short cuts and latest methods that some of the main conclusions and facts brought out by the survey can now be made:

(1) There exists a surface layer five degrees warmer than normal and one hundred meters in thickness covering an ocean area of 100,000 square miles. This additional heat reservoir of tremendous proportions is bound to have far-reaching climatic effects. This supports the assertions of many that Arctic climate has undergone recent temporary amelioration.

(2) Bottom water was found in the trough between Greenland and Labrador, the temperature of which was 2.6° C., having a salinity of 34.90. The observations showed that this water was not produced on the surface or by ice melting, as suggested in the theories of Nansen and Pettersson, but indications point to a slow bottom creep as the source of such water, possibly off the coast of Greenland.

(3) The coastal shelves of Greenland are much narrower than shown on present-day maps, while the Labrador shelf reveals itself to be wider.

(4) Three headlands sighted by the *Marion* north of sixty degrees latitude indicate discrepancies in location of the Baffin Land coast-line on the maps in some cases by as much as twenty miles.

(5) Arctic waters were extremely open. About one thousand bergs in Disko Bay near the glacier front and two hundred bergs stranded on Labrador Coast near Cape Harrison was practically the only ice present at the time of the cruise. The Arctic pack itself shrank back twenty miles off Cape Dier, Baffin Land.

The work next in importance to the determination of the set and drift of the currents and their extent was the sounding out of the sea areas covered. The Submarine Signal Company's fathometer was used for the measuring of the depths. This instrument obtains depths rapidly and accurately by timing the echoes from the sea bottom of sound waves that are sent out from the hull of the ship. It was in use day and night throughout the cruise. The result is that there are recorded over 2,100 carefully taken and located soundings from which the conclusions relative to the bathymetrical contours have been drawn. In addition to the station and sounding work, continuous records of surface temperatures and salinities were kept. All ice and other noteworthy things along the route were recorded by being drawn in on the plotting sheets.